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PROCEEDINGS  
OF THE  
LINNEAN SOCIETY OF LONDON.



122ND SESSION.

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FROM NOVEMBER 1909 TO JUNE 1910.

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LONDON:

PRINTED FOR THE LINNEAN SOCIETY,  
BURLINGTON HOUSE, PICCADILLY, W.,

BY TAYLOR AND FRANCIS, RED LION COURT, FLEET STREET.



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Publications of the Society issued during the period, 31st July, 1909, to 31st July, 1910:—

Journal (Botany), No. 270, 4th Aug., 1909.  
 „ 271, 28th Oct., 1909.  
 „ (Zoology), No. 200, 15th Dec., 1909.  
 „ 201, 22nd June, 1910.  
 „ 206, 30th Nov., 1909.

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Transactions (2nd Ser.) Botany, Vol. VII. Part XIII., Sept. 1909.  
 „ XIV., Nov. 1909.  
 „ (2nd Ser.) Zoology, Vol. X. Part IX., Nov. 1909.  
 „ XIII. „ I., Oct. 1909.  
 „ II., Feb. 1910.  
 „ III., June 1910.

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Proceedings, 121st Session, from November 1908 to June 1909:  
 October 1909.

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List of [Fellows, Associates, and Foreign Members], 1909-1910.

PROCEEDINGS  
OF THE  
LINNEAN SOCIETY OF LONDON.

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(ONE HUNDRED AND TWENTY-SECOND SESSION,  
1909-1910.)

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November 4th, 1909.

Dr. D. H. SCOTT, F.R.S., President, in the Chair.

The Minutes of the General Meeting of the 17th June, 1909, were read and confirmed.

Miss Julia Lindley, Mr. Martin Hubert Foquet Sutton, and Mr. Cecil Hallworth Treadgold, M.A. (Cantab.), were admitted Fellows.

Captain John Humphrey Barbour, M.B., Mr. Frederick James Bridgman, Mr. Linnæus Greening, Mr. Henry John Jeffery, A.R.C.S., Mr. Frank Armitage Potts, M.A. (Cantab.), Mr. Walter Theodore Saxton, M.A. (Cantab.), Mr. Hugh Scott, B.A. (Cantab.), Mr. Charles Sillem, and Mr. Charles Worster-Drought, B.A. (Cantab.), were proposed as Fellows, and Mr. Oswald Arthur Sayce as an Associate.

Mr. Richard Siddoway Bagnall was elected a Fellow.

Mr. Cecil Carus-Wilson, F.R.S.E., F.G.S., exhibited specimens and lantern-slides of the Natural Inclusion of Stones in Woody Tissue. He said:—About twenty-three years ago a gravel-pit was started in the valley-gravels occurring between Syndale and Newnham, some three miles from Faversham in Kent. Part of a wood, chiefly oak trees, covered the deposit; as the work progressed these were felled, and the stumps and roots dislodged.



The gravel consists of subangular, water-worn flints, large nodules less worn, and occasional blocks of Sarsen-stone; the whole being mixed with flint grit and quartzose sand, and forming a compact and somewhat coherent mass. Several Palæolithic implements and part of a skull of *Bos longifrons* have been found in the deposit.

The roots and stumps referred to were distributed promiscuously over the surface of the ground as the gravel in which they were embedded was removed. The work of excavating ceased in this particular part of the valley about ten years ago, so the roots still remaining have been exposed for that length of time, the others having been cut up for fuel by the cottagers in the neighbourhood.

Most of those now found there were left intact because of the large number of stones enclosed in the wood. Not only did these resist the work of saw and axe, but when burnt they burst asunder with considerable force, becoming a source of danger to those within range of the flying fragments.

The stones are actually embedded in the solid oak, and not merely included within forked portions which may have grown together subsequently. The tissue of the wood appears to have grown around the stones and enveloped them, indicating that the process was carried on under conditions of considerable pressure. There are dozens of stones embedded in some of these roots, or snags, so that the substance might be described as "a conglomerate formed of flints enclosed in a woody matrix."

In the specimen of which I now show a photograph (Plate 1) I counted no less than sixty-seven flints, the largest being several pounds in weight, and there are innumerable empty cavities showing where others existed before the shrinkage of the wood after exposure. Piles of these dislodged flints are to be seen on the ground under and around each root.

The picture now thrown upon the screen shows the same root with one of the limbs cut off to facilitate its removal to the Museum at Kew last July. Only three of the sixty-seven stones were shaken out before the specimen reached Kew—this being due to the sawing, and the shaking of the cart in which they were conveyed to Faversham.

In regard to the forked part now seen:—Each limb measures about 3 feet in length, with girths averaging about 25 inches. This part contains 50 stones. The single piece was sawn off the forked one; its length is 33 inches, and the girth measurement averages 25 inches; it contains 14 stones.

I have occasionally seen odd stones thus embedded in the trunks of trees. In Norton Churchyard, a few miles from Faversham, are three very old yew trees, and in two of them I saw flints and fragments of tiles embedded in the wood of the trunk seven feet above the ground.

In Molash Churchyard, six or seven miles south of Faversham, there are six very old and large yews. Some of these have flints



FLINTS IN WOOD.



embedded in their trunks seven or eight feet above the ground, and in one, on the north side of the churchyard, I saw flints at least twelve feet above ground.

The circumstances led me to suppose that the stones and fragments of tile had been originally pressed into the roots when these were in a soft and spongy state below ground, and that they subsequently emerged with the growth of the tree.

The examples at Syndale are, however, as far as I know, unique, and if trees can enclose stones in such quantities, and retain them within their substance so tenaciously, we have transporting agents capable, under certain conditions, of distributing terrigenous material over sea-beds to an extent not hitherto appreciated.

My thanks are due to Mr. C. Gordon Neame, of Copton Manor, for his valuable assistance in enabling me to secure the specimens referred to.

The President contributed some remarks upon the interest of this exhibition.

Dr. A. B. RENDLE, F.R.S., showed a specimen of heather (*Erica cinerea*) found near Axminster in which the flowers were replaced by dark red leaf-buds of about the same size as the flowers. One side, or about half of a clump of heather was affected; the other side bore normal flowers and the two sorts were not mixed. The red leaf-buds, which occupy the position of flowers, consist each of a number of short, strongly ascending leaves closely arranged in superposed whorls of four; the four lines have often a strong spiral twist in the upper part of the bud. The leaf-arrangement therefore resembles that of the flower, not of the foliage leaves which are in whorls of three. The leaves of these special buds also differ in form from the foliage leaves in that they are upwardly concave with a bluntly keeled back recalling the sepals of a typical flower. They are 32 or more in number, and thus considerably out-number the parts of a typical flower (24 including bracteoles). The tip of the bud was always damaged, but in many of those examined a shrivelled or more or less misshapen pistil or its parts were present, and sometimes below this semifoliateous stamens were found. The appearance suggested insect injury, but Mr. C. O. Waterhouse was unable to find any animal organism; he pointed out, however, that the appearance suggested the work of a *Phytoptus*, which in the normal course of events would have already deserted the buds. Dr. Rendle has, however, been able to find no record of *Phytoptus* in connection with our heather. The specimen is of interest as resembling a teratological form of *Erica cinerea* described by Maxime Cornu in 1879, where the flowers were replaced by vegetative buds apparently very similar in appearance to those on our specimens, but in which the arrangement of the foliage leaves was maintained (the leaves being in rows of six), while the bud



contained no trace of floral organs or of damage by any animal organisms.

A discussion followed in which the following engaged :—Mr. E. M. Holmes, Dr. O. Stapf, and the President; Dr. Rendle replying.

Prof. H. H. W. PEARSON, Sc.D., F.L.S., then gave a lecture illustrated by a long series of lantern-slides, entitled—"Types of the Vegetation of Bushmanland, Namaqualand, Damaraland, and South Angola (A Preliminary Report of the Percy Sladen Memorial Expedition in South-West Africa, 1908-1909)," of which the following is an abstract :—

The floras of the regions named in the title are very distinctly related, if the remarkable vegetation found on the Huilla plateau in South Angola be excluded. Otherwise the differences that are observed are probably to be accounted for mainly as a result of variation of (1) elevation; (2) atmospheric humidity; (3) depth at which permanent supplies of underground water are available; (4) geographical position, especially with regard to the composition of the floras of contiguous regions. In all, the rainfall is normally scanty and inconstant, and there is a prolonged drought in the winter season. Near the coast, in some places up to elevations as great as 2,700 feet, the total annual rainfall is never more than a few millimetres and frequently fails altogether.

The affinities of these floras (again excepting that of the Huilla plateau) are primarily with those of the South Central African highlands. In South Angola many species are undoubtedly derived from the Coast and Montane regions of West Tropical Africa. Throughout, the vegetation is more or less extremely xerophytic in character, and is marked either by a very short period of duration or by the possession of those structural peculiarities which are commonly found in dry climate perennials. Of these, hairiness is, in general, not a conspicuous feature; except in Lower Namaqualand, succulence is not especially common. A round bushy habit is very marked throughout. The root system is usually very deep; the leaves are commonly simple and of small size and with a strongly developed cuticle.

The formations and associations indicated are predominant by reason either of their great extent or of striking peculiarities of the plants composing them. They are arranged in the main geographically from South to North.

The President having opened the discussion, it was continued by Prof. Herdman, Dr. Henry Woodward (visitor), Dr. A. B. Rendle, Mr. Bailey Saunders (visitor), and Dr. Stapf; Prof. Pearson replying.

November 18th, 1909.

Dr. D. H. SCOTT, F.R.S., President, in the Chair.

The Minutes of the General Meeting of the 4th November, 1909, were read and confirmed.

Prof. William Bateson, M.A. (Cantab.), F.R.S., and Mr. Donald Herbert Edmund Sunder, were proposed as Fellows.

Mr. Thomas Parkin, M.A., was elected a Fellow.

Mr. F. ENOCK, F.L.S., exhibited on the screen a series of photographs of the movements of animals, and contributed the following summary of his remarks:—

Among the recent discoveries and improvements in scientific appliances connected with photography, the kinematograph stands out before all others, but it is a fact much to be regretted that ninety per cent. of the films made are for the simple amusement of the multitude. No doubt, as time goes on, naturalists will realize the immense value of the kinematograph in permanently registering movements of every kind of living creature, which can be shown time after time upon the screen. Every stage in the life-history of an insect can (with a good deal of trouble and patience) be photographed with all the detail of living movements.

During the past two years I have, with the invaluable assistance of Mr. A. Newman, taken a number of films showing the movements peculiar to certain larvæ of Lepidoptera, together with other insects, as well as those swimming in their native element. Our first film shows a beetle crawling along a stem, using its antennæ to feel its way along; a woodlouse also uses these organs for the same purpose; and a garden spider runs across so rapidly that the order in which its legs are used is quite lost in this instance.

Caterpillars of various species each show some peculiarity in their progression along a stem. That of an Ermine Moth, one of the familiar Woolly-bear type, moves very hurriedly, so that the undulatory movement is most noticeable. Others, such as the larva of the Puss Moth, show greater caution in their movements; and that of the Elephant Hawk-Moth gives a good idea of dignified motion, the bringing up and setting down of the anal segment much resembling the action of the ponderous foot of the Elephant.

The strange appendages with which the larva of *Stauropus fagi*, the Lobster Moth, moves are all shown to advantage, especially the frying-pan appendage at the tail.

As might be imagined, the undulatory movement of the "Looper" caterpillars is noticeable by its absence, owing to the fact that these larvæ have no intermediate claspers, so necessitating

the rapid drawing up of the anal claspers to the prolegs, and so making the body into a loop.

Films taken of aquatic insects such as the Common Water-Beetle (*Dytiscus*), Water-Scorpion (*Nepa cinerea*), and the Water-Botman (*Notonecta*), all show the characteristic movement of the limbs.

Perhaps the most interesting film is that showing a Butterfly emerging from its chrysalis, as it first bursts open, gradually withdraws its legs, antennae, tongue, and body, followed by the wings, catches hold with its claws as the wings fall into position, and then, swayed to and fro by the wind, mysteriously develops until the wings attain their full size.

Passing from insects, an interesting film of two Lizards engaged in a fierce struggle for the possession of a meal-worm, shows how they can plant their claws and throw each other over in the most approved fashion of wrestling.

The film of swimming Sticklebacks was produced by Mr. Newman; and the last film showed a tongueless Frog from the Cape, catching and swallowing a worm, during which operation it makes a most comical use of its front legs and claws for pushing the worm into its mouth.

I feel that attention ought to be called to Mr. Newman's invention of a safety trough, which is inserted between the illuminant and film, which, being filled with water, prevents the film from becoming dangerously heated, so that it is possible to stop the apparatus at any point, when it is necessary to explain any special feature.

Further remarks were made by Mr. John Hopkinson, Mr. E. M. Holmes, and Mr. Enock.

The following papers were read :—

1. "A New Tipulid Subfamily." By W. WESCHÉ, F.R.M.S.  
(Communicated by J. HOPKINSON, F.L.S.)
2. "Freshwater Rhizopods from the Lake District." By J. M. BROWN, B.Sc. (Communicated by Prof. A. DENNY, F.L.S.)

December 2nd, 1909.

Dr. D. H. SCOTT, F.R.S., President, in the Chair.

The Minutes of the General Meeting of the 18th November, 1909, were read and confirmed.

Captain John Humphrey Barbour, M.B., Mr. Frederick James Bridgman, Mr. Linnæus Greening, Mr. Henry John Jeffery, A.R.C.S., Mr. Frank Armitage Potts, M.A. (Cantab.), Mr. Walter Theodore Saxton, M.A. (Cantab.), Mr. Hugh Scott, B.A. (Cantab.).

Mr. Charles Sillem, and Mr. Charles Worster-Drought, B.A. (Cantab.), were elected Fellows, and Mr. Oswald Arthur Sayce an Associate.

On behalf of Dr. H. DRINKWATER, F.R.S.E., there were exhibited 25 drawings in body-colour on dark backgrounds, of wild flowers, chiefly from Wrexham.

MR. CLEMENT REID, F.R.S., F.L.S., exhibited photographs on the screen of fruits and seeds of some of the plants introduced by the Romans into Britain. The remains have been collected principally from disused Roman wells, employed subsequently as rubbish pits, and often sealed up under Roman pavements of later date. The principal sources have been Roman Silchester, Caerwent, London, and Pevensey; and to a large extent the collections have been made by Mr. A. H. Lyell, who has been most careful to reject any deposit of doubtful or later date.

The fruits and seeds exhibited belong to pea, bean, fig, grape, mulberry, medlar (a very small variety), apple, cherries (probably both black and red), sloe, bullace (wild and cultivated), damson, a larger plum like the "black plum" of Cornwall, Portugal laurel, black and white mustard, turnip? fennel, dill, coriander, alexanders, *Cherophyllum aureum* (a casual, perhaps introduced with packing-case rubbish from France, and not grown in Britain), belladonna, henbane, field poppies (*Papaver Rhoeas*, *P. Argemone*), the opium poppy (seeds of this were probably used, as in Rome, scattered on leaves of bread), greater celandine, corn-cockle, white campion, bladder campion, penny cress, sow-thistle, ox-eye daisy, *Chenopodium urbicum* and *C. murale*, and leaves of box. Box-leaves have been found in three different rubbish-pits in Roman Silchester; the branches may have been used for wreaths, as the nearest native substitute for the Italian myrtle.

The plants thus far found do not suggest any direct shipping trade with the Mediterranean. The peach, apricot, almond, and other fruits that will only ripen south of Britain are missing. The fruits and spices found are only such as can be grown commercially in Britain at the present day, and this makes it probable that the abundant fig and grape seeds belong to fruit grown in this country and not imported in a dried state. Mulberries do not travel well and are scarcely ever dried; they must have been grown at Silchester.

Mr. Lyell (visitor), Lt.-Col. Prain, Mr. G. C. Druce, Mr. L. A. Boodle, the Rev. J. Gerard, Mr. E. M. Holmes, Mr. E. G. Baker, and the President took part in the discussion which followed, and Mr. Reid briefly replied.

Mr. G. CLARIDGE DRUCE, F.L.S., exhibited specimens of (a) *Zannichellia gibberosa*, Reichb., new to Britain, from Eye Green, Northants; and (b) *Orchis maculata* var. *O'Kellyi*, Druce, from Ballyvaughan, Co. Clare, recently described in 'The Irish Naturalist.'



Mr. CLEMENT REID exhibited in connection with this, three photographs of *Zannichellia* fruits obtained by him from the Cromer Forest Bed (pre-glacial), and also contributed some remarks; Mr. Holmes brought specimens of another variety of *Orchis maculata* and commented on the same, and Mr. Druce replied.

The following papers were read :—

1. "Nudibranchs from the Indian Ocean." By Sir CHAS. ELIOT, K.C.M.G. (Communicated by Prof. J. STANLEY GARDINER, M.A., F.R.S., F.L.S.)
2. "Trichoptera von Mr. Hugh Scott auf den Seychellen gesammelt." By Dr. GEORG ULMER. (Communicated by the same.)
3. "Report on the Brachiopoda obtained from the Indian Ocean." By Dr. W. H. DALL. (Communicated by the same.)
4. "Narrative of the 'Sealark' Expedition. Part III." By Prof. J. STANLEY GARDINER, M.A., F.R.S., F.L.S., and others.

December 16th, 1909.

Prof. E. B. POULTON, F.R.S., Vice-President, in the Chair.

The Minutes of the General Meeting of the 2nd December, 1909, were read and confirmed.

Dr. William Henry Lang, and Mr. Charles Sillem, were admitted Fellows.

Mr. Cyril Crossland, and Dr. Harry Drinkwater, M.D. (Edin.), were proposed as Fellows.

Prof. William Bateson, M.A. (Cantab.), F.R.S., and Mr. Donald Herbert Edmund Sunder, were balloted for and elected Fellows.

The following papers were read :—

1. "Report on the Crustacea Isopoda and Tanaidacea collected by Mr. Cyril Crossland in the Sudanese Red Sea." By the Rev. T. R. R. STEBBING, M.A., F.R.S., F.L.S.
2. "Isopoda from the Indian Ocean and British East Africa." By the same.
3. "Pycnogonida from the Red Sea and Indian Ocean, collected by Mr. Cyril Crossland." By G. H. CARPENTER. (Communicated by Prof. W. A. HERDMAN, F.R.S., F.L.S.)
4. "On a Collection of Blattidæ preserved in Amber, from Prussia." By R. SHELFORD, F.L.S.

5. "The Bryozoa from collections made by Mr. Cyril Crossland, Part II.—Cyclostomata, Ctenostomata, and Endoprocta." By A. W. WATERS, F.L.S.

The Vice-President in the Chair announced the subject for the following meeting on the 20th January, 1910.

January 20th, 1910.

Dr. D. H. SCOTT, M.A., F.R.S., President, in the Chair.

The Minutes of the General Meeting of the 16th December, 1909, were read and confirmed.

Mr. Frederick James Bridgman was admitted a Fellow.

In accordance with the announcement from the Chair at the previous Meeting, the Meeting was devoted to a discussion upon the

### ORIGIN OF THE VERTEBRATES.

Dr. W. H. GASKELL, F.R.S. (Visitor), who opened the Discussion on the "Origin of Vertebrates," said:—I take it for granted that we all believe in Evolution and that an upward progress can be traced from the Protozoa to Man. Now the formation of the Metazoa from the Protozoa and the progress of the Metazoa upwards signifies that the separate units composing the individual have been coordinated for the well-being of that individual. Such coordination has taken place in two ways: (1) a chemical method, by the formation of hormones; (2) a nervous method, by the formation of a central nervous system, and it is self-evident that as soon as a central nervous system is formed, such nervous coordination, especially in connection with the formation of the special senses of sight and smell, must become the important factor in the life of the individual, and its further and further development must constitute the most important factor for the upward progress of the animal race. The first point I want to impress upon you is that for all questions of Evolution, the central nervous system rather than the alimentary canal is the most important factor.

Throughout the whole history of the attempts to find out the origin of Vertebrates one point stands out clearly: whatever other views have been put forward there have always been strong supporters of the view that the Vertebrates have arisen from that great group of segmented Invertebrates, the Appendiculata, and such supporters have not been outsiders of no account, but largely the main authorities in the zoological teaching of the time, *e. g.*, Geoffroy St. Hilaire, Leydig, Newport, Treviranus, Owen, Dohrn, and numerous others, all of whom based their views on the presence of the infundibulum in the Vertebrate in exactly the

same position in the brain as the œsophagus in the Invertebrate group. Supra-infundibular nerve-mass was then the same as the supra-œsophageal, infra-infundibular as infra-œsophageal, and it was seen that the function corresponded marvellously. So powerful was the fetish of the inviolability of the alimentary canal, that no one of these observers ever noticed that if the infundibulum is the old œsophagus, it leads directly into the great cavity of the ventricles of the brain, which again lead into the straight narrow canal of the spinal cord and so through the neurenteric canal to the anus; that in fact if the infundibulum is the œsophagus, the rest of the lining-walls of the cavity of the central nervous system corresponds word for word with the rest of the Invertebrate alimentary canal. On the contrary, they considered the homology could only hold good by turning the animal topsy-turvy and making the back of the Invertebrate correspond to the ventral surface of the Vertebrate. Such a method was doomed to failure and is now universally discredited.

As to the alternative hypothesis of an origin from some non-segmented Invertebrate, please think what it implies and consider seriously whether it is possible to accept it. I imagine we may take it for granted that we know the nature of all the main groups of animals alive on the earth at the present time, and as far as I know the geological record has not brought to light any forms which are not capable of being classified either among or in connection with our present main groups; yet the assumption of this hypothesis is that from some unsegmented animal low down in the scale a group of segmented animals has arisen, in which the alimentary canal was always ventral to the central nervous system and that this group gave origin to the Vertebrate. The absence of any evidence of such chain among living animals at all comparable to the well-marked evidence in the case of the Appendiculata, makes this hypothesis an improbable one; and when the hypothesis further necessitates that not only the central nervous system of such segmented animals has been built up on exactly the same lines as the central nervous system of the Appendiculata, but, contrary to all other nervous systems, has been formed hollow, and that that hollow tube has been formed in such a shape and in such a position with respect to the true nervous elements as exactly to mimic the alimentary canal of the Appendiculata with respect to its central nervous system,—I ask you plainly, does not the improbability amount to an absurdity? This I claim to be the great characteristic of the Vertebrate which differentiates it from all other animals—the presence and nature of this tube around which the central nervous system is grouped: and I beg that those speakers who follow after me and disagree with my conclusions, will give some explanation of the presence and peculiarities of this tube. To me and to all my friends who are accustomed to deal with the Vertebrate central nervous system, the explanation I have given is so self-evident and natural, that it is impossible to look at the matter in any other way.

The paramount importance of the development of the central nervous system for the upward progress of the members of the Animal Kingdom leads to the conclusion that each higher group of animals has arisen in succession from the highest race developed up to that time, by highest meaning the group possessing the best developed central nervous system. This law is proved to us most clearly by the evidence of the rocks in the case of the Vertebrate group.

Thus we see that Man came from the Mammals, the highest race in the Tertiary times. They arose from the Reptiles, the highest race in Mesozoic times, who in their turn arose from the Amphibians, the lords of the Carboniferous epoch. Further back we leave the land and find that the Amphibians arose from the Fishes, the earliest of the Vertebrate race which swarmed in Devonian times. This steady sequence in upward progress from Fishes to Man, revealed by Geology in the long series of ages from the Devonian to recent days, is in absolute conformity with the upward development of brain-power through the Vertebrate series from Fishes to Man, as shown by the investigations of Comparative Anatomists, especially Edinger and Elliot Smith.

If thus it can be proved that such a law of Evolution has held good through the enormous spaces of time between the beginning of the Devonian and the present day, surely it is highly probable that the same law has held throughout, and that therefore the Fishes themselves arose from the race that was the most highly developed at the time when they first appeared: a race therefore which possessed a central nervous system most closely resembling that of the fish.

The evidence of the rocks points to the Silurian age as the time when the Vertebrate first arose, and to the great and striking group of Arthropods which swarmed in the seas at that time, to which the name Palæostraca has been given. These were the highest developed race at that time and from them, according to this law of Evolution, the Vertebrate ought to have sprung.

The great problem then for the study of the origin of Vertebrates resolves itself into this: What was the nature of the earliest fish and of the Palæostraca in Silurian times?

That was the problem I set myself, and it is that comparison which I have attempted organ by organ in my recent book. Such an attempt was rendered possible by the fortunate occurrence of one of the Palæostracan Group—*Limulus* or the King Crab—being still living in the present day, and what is still more important, the remarkable resemblance of *Ammocoetes*—the larval form of the Lamprey—to the fishes belonging to the Osteostraci, especially the close resemblance in position and structure of that remarkable muco-cartilaginous head-shield of *Ammocoetes* to the head-shield of such a fish as *Cephalaspis*.

My object throughout has been by the study of *Ammocoetes* to find out a clue to the past history of these extraordinary early forms of fish. The results are published in my book, and give a



striking evidence of the way in which these early fishes may have arisen from their contemporary Palaeostracan rivals. It must always be remembered that these latter animals were not Crustaceans or Arachnids, but the precursors of both of these groups, and much nearer to their origin from the Annelids than the present day Arthropoda. To this circumstance must be attributed the annelid characteristics so markedly found in the Vertebrate, especially in the excretory organs.

It seems to me highly probable that this same law of upward progress, viz., that each successive group has arisen from some member of the highest group existing at the time, holds good also for the vegetable kingdom, especially in view of the statement recently made that Phanerogams arose from Cycads. I hope that the President may see his way to offer a few remarks on that aspect of the question.

The great stumbling block to the acceptance of my theory in the minds of many, is the necessity of making a new digestive tube in a highly organised animal, and yet the same zoologists accept without the slightest difficulty, as a commonplace, the manufacture of a new respiratory organ for breathing air instead of water in the transition from the fish to the amphibian. The previous factor in that case was the swim-bladder which provided the new organ, in the other a respiratory chamber formed by the internal gills: for one of the great characteristics of many members of the Palaeostracan group is the absence of external gills and the indication of internally situated gills, and it does seem to me that the evidence is stronger in favour of the Vertebrate alimentary canal being formed from a preexisting respiratory chamber, than that an alimentary canal should have taken on a respiratory function in its anterior end.

The way in which the alimentary canal is innervated by the downgrowth of the great respiratory nerve, the vagus, which is so clearly a segmental nerve for the respiratory part but not for the small intestine, points to this conclusion. The fact that in the well-marked segmental respiratory chamber of *Ammocoetes* a new unsegmented alimentary tube should be formed at transformation, again indicates that a segmented respiratory chamber was the precursor of an alimentary canal. Finally, the position of the anus in such a form as *Drepanaspis* and *Bothriolepis* immediately following upon the region of the head-shield, suggests strongly that in these most ancient and extraordinarily formed fishes the anus followed close upon the mesosomatic or respiratory region just as it does in such an animal as *Limulus*.

Finally in this sketch, not of details but of general principles, I come to the argument that this theory is untenable because it contravenes the fundamental principles of ontogeny.

Against this statement I most strongly protest, for the strength, I might almost say the main strength, of my position is based on the facts of Vertebrate development.

The one great principle of ontogeny is the Law of Recapitulation,

the law which lays down that the past phylogenetic stages which have led to the evolution of any individual are indicated to some extent in the ontogeny of that individual.

This law is confirmed and indicated in a most amazing way by my theory. The theory asserts that the clue to the origin of Vertebrates is to be found in the tubular nature of the central nervous system of the Vertebrate in that the central nervous system is in reality formed of two things: (1) a central nervous system of the Arthropod type, and (2) an epithelial tube in the position of the alimentary canal of the Arthropod.

Is it possible for embryology to recapitulate such a phylogenetic history more clearly than is here the case? In order to avoid all possibility of our mistaking the clues, the nerve-tube in the embryo always opens into the anus at its posterior end, while in the larval *Amphioxus* it is actually still open to the exterior at its anterior end. Consider the shape of the nerve-tube when first formed in the Vertebrate. At the cephalic end a simple bulged-out tube with two simple anterior diverticula, which passes into a narrow straight spinal tube: from this large cephalic bulging a narrow diverticulum, the infundibulum, passes to the ventral surface of the forming brain. This tube is the embryological expression of the simple dilated cephalic stomach, with its ventral oesophagus and two anterior diverticula, which opens into the straight intestine of the arthropod. Nay more, by its very shape and the invariable presence of two anterior diverticula, it points not only to an Arthropod ancestry but to a descent from a particular group of primitive Arthropods. Then comes the formation of the cerebral vesicles and the formation of the optic cup, telling us, as plainly as can be, how the invasion of nervous material over this simple cephalic stomach and its diverticula has altered the shape of the original tube and more and more enclosed it with nervous elements.

So, too, in the spinal cord region. When the tube is first formed, it is a large tube, the latero-ventral part of which presents two marked bulgings; connecting these two bulgings is the anterior commissure. These two lateral bulgings, with their transverse commissure, represent with marked fidelity the ventral ganglion masses of the Arthropod with their transverse commissure, and occupy the same position with respect to the spinal tube, as the ganglion-masses do with respect to the intestine in the Arthropod. Then the further development shows how, by the subsequent growth of the nervous material, the calibre of the tube is diminished in size and the spinal cord is formed.

Again, I say, is it possible to conceive that embryology should indicate the nature of the origin of the Vertebrate nervous system more clearly than it does?

It is the same with all the other organs. Take for example the skeletal tissues. The study of the Vertebrate embryo asserts that the cartilaginous skeleton arose as simple branchial bars and a simple cranio-facial skeleton, and also that the parenchymatous

variety of cartilage represents the embryonic form. Word for word, the early embryonic stage of the Vertebrate skeleton closely resembles the stage reached in the Arthropod, as shown by *Limulus*, and again records unmistakably the past history of the Vertebrate.

So, too, with the whole of the prosomatic region; the situation of the old mouth, the manner in which the nose of the Cephalaspidian fishes arose from the Palæostracan, are all shown with vivid clearness by Kupffer's investigations of the early stage of *Ammocoetes*, while at the same time the closure of the oral cavity by the septum shows how the oral chamber was originally bounded by the operculum. Nay, further, the very formation of this chamber embryologically was brought about by the forward growth of the lower lip, just as it must have been if the chilaria grew forward to form the metastoma. So, too, the study of the embryo teaches that the branchiæ arise as ingrowths, that the heart arises as two longitudinal veins, just as the theory supposes from the facts provided by *Limulus* and the Scorpions.

No indication of the origin of the thyroid gland is given by the study of its structure in any adult Vertebrate, but in the larval form of the Lamprey there is still preserved for us a most graphic record of its past history.

The close comparison which it is possible to make between the eye-muscles of the Vertebrate and the recti muscles of the Scorpion group on the one hand, and between the pituitary and coxal glands on the other, are based upon, or at all events are strikingly confirmed by, the study of the coelomic cavities and the origin of these muscles in the two groups. In fact the embryological evidence of the double segmentation in the head and the whole nature of the cranial segments, is one of the main foundation stones on which the whole of my theory rests.

So it is throughout. Turn to the excretory organs: it is not the kidney of the adult animal which leads direct to the excretory organs of the primitive Arthropod, but the early embryonic origin of that kidney.

So far from having put forward a theory which runs counter to the principles of embryology, I claim to have vindicated the great Law of Recapitulation which is the foundation stone of embryological principles. My theory is largely based upon embryological facts, and its strength consists in the manner in which it links together into one harmonious whole the facts of Embryology, Palæontology, Anatomy, and Physiology.

It cannot then be said that my theory contravenes this great law of development, the Law of Recapitulation. What, then, is the objection? It is that it disregards the germ-layer theory, a theory which assumes that the origin of the Metazoa from the Protozoa took place by the formation of a gastrula-form—Hæckel's hypothetical Gastræa—which gave a fixed and definite morphological origin to hypoblast, and that from that time up to the latest animal development that hypoblastic layer has always

remained the same. Such a positive assertion, if true, immediately puts out of court any theory which forms an alimentary canal out of something which is not hypoblast. It makes the alimentary canal the keystone of the whole fabric of Evolution, not the central nervous system.

As I have pointed out in my book, the evidence of Brehm and others is to the effect that there is no such morphological criterion of hypoblast, but, on the contrary, the hypoblast is a physiological conception rather than a morphological one, being the term given to that layer which is found by its development to form the digestive tube of the animal, and that in the earliest members of the Metazoa, where we ought to expect the gastrula formation to be most conspicuous, there it is most conspicuously absent, while it is most clearly evident in those free-living pelagic blastula-forms in which, owing to the absence of yolk, the necessity exists of obtaining food from the outside even from the early blastula stage.

According to the Law of Recapitulation we may expect to find in the developmental history of the Metazoa some indication of the nature of the Protozoan ancestor which gave origin to the Metazoa. Such indication is given with absolute uniformity in all the Metazoa by the blastula stage, not by the so-called gastrula stage. The blastula represents one of the highest Protozoan forms, such, for example, as *Volvox*, as I have suggested in my book, and the blastula stage affords yet another indication of the great law, that the upward progress of the Animal Race has always been brought about by the genesis of the next highest form from a member of the highest existing group of animals.

Prof. E. W. MACBRIDE, F.R.S. (Visitor), remarked:—

Dr. Gaskell has given us a brilliant exposition of his famous theory of the "Origin of Vertebrates" to which it is impossible to reply at all adequately in a quarter of an hour. Fourteen years ago this theory was presented to the Cambridge Philosophical Society and I then gave expression to many objections which I felt to it; and I confess that those objections remain in unaltered force to-day. Not one of them has been removed by Dr. Gaskell's speech, nor has a perusal of the latest edition of his book weakened one of them in the slightest degree.

The first and most fundamental objection is to the whole nature of Dr. Gaskell's morphological reasoning. Unless this kind of reasoning is to be guided by definite rules it becomes a mere arena for the display of the imaginative faculties. The change which one man regards as inconceivable another thinks the most natural in the world. I, for instance, cannot contemplate in cold blood a free-living animal giving up its alimentary canal and beginning to digest with its skin, whilst to Dr. Gaskell this seems the most natural transition in the world. But what rules for morphological reasoning are suggested? Tacitly or avowedly, all zoologists agree on this—morphological reasoning must conform to precedent. But what constitutes precedent in this case?



Those changes about the nature of which all zoologists are agreed, such as the relationship of an aberrant genus to the typical form of the family or order to which it belongs. Thus no one doubts that the Hermit Crab is descended from a normal Lobster or *Chatopterus* from a normal Annelid. The changes involved in the descent of such forms from the more normal types give us the only rules we can have to guide us when we attempt the more difficult task of passing from one phylum to another.

Now Dr. Gaskell, in assuming that Vertebrates are descended from some Pakeostracan type of Arthropod of which the only survivor is *Limulus*, is obliged to reconstruct the entire animal, leaving only the central nervous system standing. We are asked to believe that the original alimentary canal has become the neural canal, and that a new alimentary canal has developed from the skin of the ventral surface of the body. No precedent for such a change can be gathered from any of the data I have mentioned above.

Again, the skin of the lower Vertebrates is ciliated, and this is most undoubtedly a primitive condition seeing how widely it is spread amongst the lower groups in the Animal Kingdom. No Arthropod\* is ciliated at any time of its existence: its whole organisation is dominated by the tendency to form thick chitinous cuticle. We have to suppose that this tendency, which is spread throughout Arthropoda from the highest to the lowest, has been overcome and that a reversion to a primitive soft ciliated ectoderm has been accomplished. No precedent for such a change can be gathered from the entire Animal Kingdom. It is no answer to this to show that in *Ammocoetes* and one or two other cases a thin exterior cuticle is developed on certain parts of the skin—for it is the normal sequence of things that a cuticle should succeed to a ciliated skin as a secondary change, but the change in the reverse direction is absolutely without precedent.

The eyes of Vertebrates, or, to speak more correctly, their retinae, are lateral pockets of the walls of the neural canal—which we are told to regard as the old alimentary canal. The eyes of Arthropoda are, without exception, modifications of the external skin. Are the lateral eyes of the two groups homologous or are they not? If they are homologous, how is their different origin explained? Dr. Gaskell figures a section of *Artemia* in which one of the liver sacculi is in close contact with the lower layer of the eye. He hints that perhaps part of the eye is developed from the epithelium of the liver sacculi, but this is in flat contradiction to the work of every zoologist who has examined their development. If the eyes in the two cases are not homologous, why did the Arachnid ancestor of Vertebrates give up its external eyes and develop a new pair from its old alimentary canal? To say that there is no precedent for such a change is to put it mildly.

\* I hardly think it necessary to refer to the ciliation of the genital ducts of *Peripatus*, the only exception to this rule, since *Peripatus* is hardly as yet an Arthropod.

Dr. Gaskell indulges in a polemic against the germ-layer theory, whilst maintaining strongly the theory that the development of the embryo recapitulates the history of the race. He seems to be unaware that the germ-layer theory is only a special instance of the recapitulation theory. It asserts that the egg in its progress to a hollow blastula recapitulates the change from a unicellular to a multicellular Protozoon. This part of it Dr. Gaskell accepts, and with justice, for in the development of simple and primitive types the blastula crops up throughout the entire Animal Kingdom. But in every case which is free from the complication of yolk, the blastula is transformed into a hollow gastrula by a process of invagination, so that we find that in the Arthropod *Lucifer* and the Vertebrate *Amphioxus* the process is very similar. And yet Dr. Gaskell asks us to believe that in the one case the cavity is homologous with the neural canal of Vertebrata and in the other with the gut! Such reasoning seems to me to be very difficult to accept. Dr. Gaskell assumes that *Lucifer* developed the hollow gastrula stage because its egg is a small one floating in water and has to absorb nourishment early through the blastopore, but the fact is that in this stage of its development the egg of *Lucifer* is in a tough shell and that before it begins to absorb nourishment the blastopore closes, and this is the case with *Amphioxus* also. The doubts as to the validity of the germ-layer theory have concerned themselves chiefly with the nature of the third layer, the mesoderm. If everything which is found between ectoderm and endoderm be called mesoderm no doubt confusion will arise, for heterogeneous structures are confounded under this name. But the more careful investigation of doubtful cases in recent years, for which we are specially indebted to the Americans, have shown that if by mesoderm we mean the *wall of the celom*, then this is homologous in all cases and always arises from the gut-wall.

I pass over minor difficulties of Dr. Gaskell's theory, such as the degeneration of the ancient genital gland into packing tissue surrounding the brain, and the transformation of the womb into a gland which in *Ammocetes*, as in *Amphioxus*, produces a string of mucus to entangle food; for the mind which accepts the main ideas of the theory will be capable of digesting such trifles also. We come then to the only points in Dr. Gaskell's theory which in the mind of any zoologist would constitute even *prima facie* evidence in its favour, viz. the external resemblance between some of the armoured fish of the Devonian and the contemporary Eurypterids, coupled with the assertion that when Vertebrates appeared Arthropods were dominant in the water, and that only creatures with strong armour and well-developed nervous systems could have overcome them. Dr. Gaskell infers that if the primitive Vertebrates had been like *Amphioxus* they never could have won the day. Now to this, two answers may be made. First, that the resemblance is purely superficial, in fact far less than exists between a Whale and a Fish: we should in fact have far more reason for classing Whales as Fish than for regarding *Cephalaspis* as allied

to *Limulus*. This point I shall leave for elaboration to the palæontologists who follow me. Secondly, Dr. Gaskell has no right to assume that *Cephalaspis*-like forms were the first Vertebrates. It is entirely to ignore all that Darwin taught on the imperfection of the geological record, and already the discovery of forms like *Thelodius* with a skeleton of isolated denticles and of fusiform fish like *Birkenia* and *Lasanius*, in which the skeleton, if any, was formed of small isolated plates, has given the lie to such assumptions. If early Vertebrates were like *Amphioxus* they may have existed from Pre-Cambrian times and we should have found no trace of them. Moreover, the form of *Cephalaspis* and its allies is totally unlike the typical fish form. This is fusiform and flattened in the vertical plane, while *Limulus*, like all Palæostraca and the overwhelming majority of Arthropoda, is flattened in the horizontal plane. *Cephalaspis* in outer form resembles such modern fish as *Lophius* (the Angler) and the Gurnards, which habitually squat on the bottom and some of which bury themselves in the mud; and some of these forms actually develop their scales into plates and have their eyes shifted dorsally. I have no doubt at all that whilst *Cephalaspis*, *Pterichthys*, and their congeners were practising this sluggish mode of life, the real ancestors of the dominant Vertebrates of the sea were ranging like flashes of living light through the waters above. It is customary to speak of *Amphioxus* as a degraded creature, but no one who has ever seen it swim will fail to realize the immeasurable superiority of the Vertebrate motor system over that of the Arachnid. The comparison of the one to the screw of a steamer and of the other to an eight-oared boat gives some idea of the difference. We may add that the whole course of evolution in fish and other Vertebrates has tended in the direction of getting rid of external armour, and there is no foundation for Dr. Gaskell's assumption that the possession of heavy external armour indicates a "dominant" form. It really indicates a sluggish form. But Dr. Gaskell continues, there is the unique feature that the Vertebrate nervous system is tubular and that the ganglion cells bear the same relationship to this tube as do the ganglia of an Arthropod to its alimentary canal, and the central nervous system is the most important organ in the body: whilst all else may change it endures. Here again every single item of this statement may be met with a denial. A tubular nervous system is not confined to the Vertebrata. It is found amongst the Echinodermata in Ophiuroidea, Echinoidea, and Holothuroidea, and in all cases it is formed precisely as in *Amphioxus*. An exposed plate of nervous ectoderm, such as is found throughout life in Asteroidea, is covered by the meeting of two thin non-nervous flaps. Then again the ganglionated character of the nervous system of an Arthropod is appealed to as a sign of high differentiation—but this is entirely to misread it. This character depends on the nature of the locomotor system, which consists of discrete groups of muscles confined to appendages, which leads to a local grouping

of motor nerve-cells. What intelligence *Limulus* has is confined to its minute archicerebrum, and this is probably small in amount. *Amphioxus* has no particular reason to fear *Limulus* on the ground of brains. In the Vertebrate the swellings of the nervous system are associated with the development of large sense organs, but its locomotor organs are the almost continuous bands of muscle known as myotomes, and hence the motor nerve-cells form a practically continuous plate. Moreover, the whole study of the Animal Kingdom is dead against the assumption that all else may change but the nervous system must endure. If we start with the most highly developed Arthropoda, or with the most highly developed Mollusca, we find as we pass back to more primitive forms that the nervous system evaporates into a mist of general ciliated nervous ectoderm. Out of this, as required by the exigencies of motor and sensory organs, accumulations of nerve-cells develop, and disappear with the disappearance of these organs. Of course, like every other organ, when they have persisted for a long time in a phylum they become stable, but why we should trace the highly developed brain of a Cuttlefish back to primitive ectoderm and pass from the developed nervous system of a typical Arthropod to the typical nervous system of a developed Vertebrate—ignoring all the really primitive forms belonging to the Vertebrates, is conceivable to no one who really knows zoology.

The alternative theory to his, as Dr. Gaskell admits, is that Vertebrates arose from some simple form with undifferentiated organs. *Amphioxus* gives us an idea of the Vertebrate structure in its most undifferentiated form, but showing the characteristic Vertebrate organs of notochord, gill-slits and tubular nerve-cord. The worm-like *Balanoglossus* and its allies show the same structures, but without the segmentation characteristic of the muscles of *Amphioxus* and other Vertebrates. But in its development, which shows far more primitive features than that of any known Arthropod, *Amphioxus* resembles *Balanoglossus*. The larva of *Balanoglossus* resembles that of Echinoderms, and here we have a hint of a wide ranging free-swimming group of pelagic animals, the direct descendants of whom are Vertebrata, but the degenerate off-shoots of which at various levels are Echinodermata, Enteropneusta, *Amphioxus*, and Ascidians.

Dr. Gaskell heaps scorn on the idea that Vertebrates, the dominant class, arose from a degenerate like *Balanoglossus*, and asks how such worms could have competed with the big Arthropods. No one supposes that Vertebrates are descended from *Balanoglossus*, but at the immensely remote period of time when the ancestors of *Balanoglossus*, leaving their closely allied compeers the ancestors of Vertebrata, deserted the surface to seek the mud, the ancestors of the Gaskellian Arthropods were probably in the condition of the Trochophore larva.

Dr. Gaskell alludes to Spengel's work on *Balanoglossus* as destroying the supposed Vertebrate character. Nothing could be more mistaken. Every argument of Spengel has been



pulverized, and every statement of Bateson confirmed in the sixteen years that have succeeded the publication of Spengel's work.

Dr. Gaskell calls the theory of "parallel development," by which he means the theory of the independent origin of the great phyla Arthropoda, Mollusca, Vertebrata, &c. from simple forms, an "unscientific and inconceivable suggestion." Surely he has forgotten the 'Origin of Species.'

Does he forget that Darwin felt the differences between these phyla so strongly that he doubted their common origin, and seems to have imagined that they might have originated independently from primordial protoplasm. Does not Dr. Gaskell know that those who give their lives to the study of Zoology have "parallel development" or fan-like development forced on them at every turn, in every section and sub-section of the Animal Kingdom. That the air-breathing type of gastropod Mollusc, for instance, must have originated at least half a dozen times and the snake-like Vertebrate at least a dozen times each time in entire independence of every other. And why unscientific? If protoplasm be fundamentally the same sort of thing at bottom, and if variations be due to definite changes in its chemical composition produced directly or indirectly by changes in the environment, should not like causes have like results?

Dr. Gaskell states that his theory strikes at the root of the conception of parallel development. In this case I venture to predict that the root will prove to be more resistant than the axe with which it is struck.

Prof. E. H. STARLING, F.R.S. (Visitor), followed and remarked:

I do not know how far an apology may be considered necessary for the intervention of a physiologist in the discussion of a topic which has hitherto been regarded as the special preserve of the zoologist and comparative anatomist. I understand, however, that the chief criticism of the theory, which has been so ably put before us this evening, has had reference to the method by which the problem is attacked, rather than to the facts in comparative anatomy which have been discovered or collated by Dr. Gaskell. On this point, namely, the principles which must guide any research into the phylogeny of our race, a physiologist has as good a right to be heard as has a comparative anatomist. In fact, it was the author of the 'Origin of Species' himself who introduced physiological considerations into the theory of descent. Darwin showed that the grouping of living beings made by zoologists had a far deeper significance than mere resemblance of form, and were really expressions of blood relationships among the members of any group or between allied groups. He thus replaced a purely conceptual anatomical grouping by an actual physiological kinship. Since the varying degrees of divergence among different forms are to be referred to the survival only of such individuals as are most

fitly adapted to their environment, the problems of relationship, of descent and, in short, of the origin of species become part of that great study of adaptation which is the proper occupation of the physiologist. These problems are bound up, not with the outward seeming of an organ or organs, but with their *use* to the animal in the struggle for existence, and are therefore in the first place problems of function.

In a search for the ancestry of Man and of Vertebrates generally we must therefore remember that we are dealing, not with museum specimens, but with living organisms, and must endeavour to learn what are the essential factors in the life of the animal that give it an advantage over its fellows and tend to the perpetuation of its stock.

We have really two questions to deal with, namely :—

- (1) What determines survival of type ? and,
- (2) What determines dominance of type ?

*Survival* is merely a question of perfection of adaptation and does not necessarily imply that the type which survives becomes dominant. There are many holes and corners on the surface of the globe where the environment is of a very special character, and in each of these we shall find some group of organisms adapted for this environment and for none other. In many cases such an environment is furnished by the surface or interior of some other type leading a more active existence. It is in this parasitic condition that we get the most extreme degree of specialized adaptation associated with degeneration of all parts rendered unnecessary by the restricted range of environmental events to which the organism is liable.

*Dominance* of a type, on the other hand, involves wide distribution and, in most cases, the existence of numerous species of the same general characteristics under widely different conditions of environment. To such a dominant type belongs the Vertebrate with its highest representative, Man. There can be no doubt that the evolution of such a type must have been continuous and progressive. It has often been imagined that the evolution of the dominant forms of life was simultaneous and not successive, and was to be compared rather to the spokes of a fan than to a tree with its branches diverging from a common stem. Such a fan-like evolution could only occur with a complete separation of environments. It is as difficult to conceive that the Vertebrate was evolved from a primitive worm-like organism which shot up past the more highly developed Arthropoda, as it is to believe that mankind is destined to be replaced by some beast that is now being evolved from lower groups in the depths of the sea. But what do we mean by speaking of lower and higher groups ? The idea involved in this antithesis is the same as that included in the term "dominance." The position of any type in the animal scale, the question whether it is to win in life's struggle, is determined

by *range of adaptation* or of reaction. The organ or system on which the range of adaptation depends is the one on which we must concentrate our attention in tracing back the evolution of the Vertebrate. This organ is the central nervous system. There has been no continuous rise in type of the muscular, digestive, or respiratory systems. It is the central nervous system which determines dominance of any type, and the nervous system is the only part of the body which undergoes continuous evolution from the lowest to the highest forms. The reactions of the highest animals are determined by the nerve-cells and tracts laid down in the embryo and inherited from the parents, no new formation or repair being possible after the earliest stages of foetal life, if indeed at any time. In no case, so far as I am aware, do we find the central nervous system cleared away and laid down afresh in the metamorphosis of an animal. At various times an animal may breathe by its skin, by gills or by lungs. It may digest its food by means of glands derived from the epiblast or hypoblast, and indeed digestive ferments may be produced by almost any cell in the body. It may excrete waste products by kidneys, intestines, or skin; but the central nervous system remains the one unchangeable organ, whose function, namely, the determination of adapted reactions and therefore of survival, cannot be replaced by the vicarious activity of any other part of the body.

Looking back as physiologists we may indeed see that all the main epochs in the evolution of higher forms of life are characterized by changes in the nervous system. The first step was taken when the individuals of a cell colony remained in structural connection, so that the *consensus partium* could be maintained by the propagation of molecular changes along the protoplasmic strands between the different cells and no longer depended solely on the diffusion into the surrounding medium of chemical substances which might affect friend or foe alike. By a differentiation among these connecting strands a diffuse nervous system was formed with immensely enhanced rapidity of reaction of the whole organism to environmental changes at any part of its surface. The location of the mouth at the front end of the body, *i. e.* the one which in the actively moving animal was first exposed to changes in the environment, was attended by the concentration at this end of the specialized projicient organs of sense, *i. e.* those whose activity was aroused by changes occurring at some distance from the animal, in a region with which a continuation of the forward progression of the animal would bring it in more intimate relations. The presence of these *foreseeing* organs at the anterior end necessarily brought in its train a subjection of all other parts of the nervous system to that part, the supra-oesophageal ganglion, which was the first recipient of the afferent impressions from these organs. The rise in type, which has culminated in the production of Man himself, has been determined simply by a continuous

advance in the complexity of adaptations, and by an increase in the powers of control and foresight exercised by the foremost part of the central nervous system. On these two factors, foresight and control, depends a man's position among his fellows, and a continuous growth in the same factors marks the progression of living forms from the Worm to the highest Vertebrate.

Since the functions which determine survival are those bound up almost exclusively with the central nervous system, this system is taken by Gaskell as his guide in tracing the genealogy of the Vertebrate. I am not sufficiently equipped to bear testimony in favour or otherwise of the facts adduced by Gaskell in support of his theory. I am convinced, however, that the principles on which he has proceeded are the only ones which will lead to a solution of the problem, and that researches along these lines will throw light on the meaning and physiological significance of many organs whose part in the economy of the body is still a mystery. It is difficult to understand the attitude which has been taken up by the majority of zoologists towards this theory of the origin of Vertebrates. We find zoologists themselves putting forward theories of the descent of Vertebrates based on a more or less profound study of all sorts of organs and structures which really have little or no importance in the life of the animal, or can be replaced vicariously or structurally with the utmost ease. Thus they concentrate their attention on organs such as the alimentary canal, blood vessels, fœtal membranes, excretory organs, the notochord, but pay little or no regard to the one system of the body which is all-important in determining the continuous series of adaptations which make up the life of the animal. And what is strange is that in most cases no palæontological evidence seems to be brought forward in favour of these hypotheses. I do not know whether succeeding speakers will be able to adduce any facts from the geological record in favour of the existence of the strange slug-like animals, with or without holes punched in them, which have been evolved out of the inner consciences of our most distinguished zoologists and assigned to us as our remote ancestors. To an onlooker like myself the striking resemblance between the earliest fishes and the Arthropoda which were the dominant type just before the appearance of these Vertebrates, is striking evidence in favour of Gaskell's theory. I would ask the morphologists present here to-night to explain how they account for this striking similarity. If the gastrula theory had been mentioned in the first chapter of Genesis, it is possible that the presence of those earliest fishes in the earth's crust might be regarded as a divinely appointed trial of faith for the orthodox among zoologists. It seems to me that the morphologist, while professing a lip service to the doctrine of Evolution, has really forsaken the teachings of Darwin and gone back to the worship of his old idol, the study of form for itself. For him, as for the anatomists before Darwin, similarity of form is everything and function is of no account.



The special message of Darwin to biology was the vindication of function, and the demonstration that it was the use of parts and not their shape which determined their significance,—that relationship between different types was a question of descent and of survival, and therefore depended not on form but on fitness, that is to say, on physiological function. It is curious to note, with this relapse into scholasticism, the old tendency to intolerance of new ideas and of any light on the problems at issue other than that shed by some enshrined man-made theory at the end of a dark passage. In fact some members of the zoological hierarchy apparently regard the attempt to throw light from any other direction as impious, and associate it, like many worthy divines did the work of Darwin, with the Author of all evil. I would not however like to suggest that Professor MacBride entertained any such comminatory feelings or was conscious of any spirit of religious intolerance when he speaks of the “diabolical ingenuity” of Gaskell’s theory. But surely the *odium theologicum* is out of place in dealing with biological problems. A sacerdotal attitude of mind will never advance our knowledge of natural phenomena or of the origin of Vertebrates. It is a happy augury for the revival of freedom of thought in English biology that the Linnean Society should, in this jubilee year of Darwin, have devoted an evening to the discussion of a theory, which, I believe, will prove to be the most important contribution to the history of our race since the publication of the ‘Descent of Man.’

Mr. E. S. GOODRICH, F.R.S., F.L.S., stated that before embarking on a theory as to the origin of the Vertebrates, we may attempt to determine what must have been the structure of the primitive early Vertebrate from which the Cephalochorda, Cyclostomata, and Gnathostomata (Fish and higher Vertebrates) have been derived. That all these forms are bilaterally symmetrical coelomate animals, provided with gill-slits, notochord, and dorsal central nervous system, will be granted to start with; but we must further try to find out what has been the general course of differentiation and specialization, to distinguish the higher from the lower forms, and to point out what other characters must have been absent or present from the undifferentiated ancestral stage common to them all.

With considerable certainty Gnathostomes can be traced back to an aquatic fish-like ancestor, in general structure not unlike the modern Selachian. It possessed biting jaws with true teeth, a general covering of denticles, open branchial slits, paired and median fins, a cartilaginous endoskeleton, and well-developed sense-organs.

The Cyclostomes belong to an altogether lower grade of organisation, the primitive characters of which cannot be merely due to degeneration. The segmentation of the body is more complete, and the segments are more uniform. This is especially the case

in the head. The formation of a distinct head-region with a large differentiated brain, a skull, and cranial nerves, is one of the most important and characteristic features of the structure of the Craniata (Cyclostomes and Gnathostomes). It takes place by the gradual modification of more and more of the segments at the anterior region of the body where are situated the mouth, gill-slits, and paired organs of sense. But this process of cephalization has gone much further in the Gnathostomes, where the 9th and 10th cranial nerves become included in the skull, and the corresponding muscle segments are suppressed, than in the Cyclostomes, where these nerves emerge behind the rudimentary skull and the muscle segments still in the adult form an uninterrupted series from in front of the mouth to the tip of the tail. Moreover in the Cyclostomes there are no paired limbs, no true teeth, in fact no trace whatever of dermal skeleton, and the testis has not yet acquired any direct connection with the kidney tubules.

The next point to be studied is the structure of the common ancestor of the Cephalochorda and the Craniata. Now, although *Amphioxus* is doubtless in some respects a very specialized animal—as for instance in the possession of an atrial cavity—yet it preserves many primitive characters. Judging from its structure, we must conclude that the ancestral Vertebrate was still more uniformly segmented than the primitive Craniate. The head-region was scarcely differentiated at all, there was no skull (probably no cartilaginous axial skeleton at all), a quite rudimentary brain, no specialized cranial nerves, no cephalization due to the presence of large paired organs of sense. It is possible that *Amphioxus* is somewhat degenerate; but it cannot seriously be urged that it once possessed in well-developed condition those paired sense-organs which have so profoundly modified the structure of the head-region in the Craniata. For it would be ridiculous to suppose that the modified segments could be restored to their original condition of uniformity with the trunk segments; no trace of the disturbance appearing in either adult or embryo.

Further, in *Amphioxus*, there is no dermal or epidermal armour, and primitiveness is shown in the structure of the endostyle, which becomes modified into the thyroid gland in higher forms. Lastly the presence of true nephridia, a type of excretory organ which has been lost in other Vertebrates, links *Amphioxus* to the lower Invertebrate Cœlomata.

Thus can be traced an irreversible series of stages in the differentiation of Vertebrate structure, at the bottom of which we find a much simpler, but still essentially Vertebrate ancestor, probably already extinct in Silurian times.

Among the various Classes of modern Invertebrates we do not, and indeed cannot expect to find any close allies. But the somewhat distantly related Enteropneusta (*Balanoglossus*) seem to

point to a remote common ancestor in which the supporting notochord was not yet formed, the nervous system was superficial and more diffuse, and the segmentation less perfect.

We have seen that the study of the Vertebrates leads us back step by step to a simple undifferentiated ancestor, in which the complex sense-organs, the highly developed brain, the chambered heart, and other structures so characteristic of this phylum had not yet appeared. Now, the same conclusion is reached on studying such other groups as the Mollusca and Arthropoda. Here also we are led back along an irreversible series of forms to a simpler generalized ancestor. The Vertebrates, Molluscs, and Arthropods, have diverged along fundamentally different lines of differentiation.

Just as the organisation of the Vertebrata is governed by the appearance of a dorsal nervous system, a notochord, gill-slits, a mesoblastic skeleton, etc., so the whole organisation of the Arthropoda is dominated by the secretion of a complete superficial chitinous skeleton, and the accompanying development of jointed appendages serving for feeding and progression. Similarly, the Molluscan organisation has been throughout influenced by the secretion of a calcareous dorsal shell, and the development of a soft body capable of distention by the blood-vascular system. Of all the systems of organs the nervous system may be considered as the most important, and it is just in the study of this system that we can most easily trace the divergence in structure of the three groups.

Owing to adaptation to similar environment or function certain striking resemblances may occur between animals of widely separated origin; this is especially the case with sense-organs adapted to receive definite stimuli. Thus, a Cephalopod Mollusc has a large brain enclosed in a cartilaginous skull, with paired orbits containing large eyes remarkably like those of the Craniate Vertebrate. But the resemblance is due to convergence; these complex organs were not present in more primitive Mollusca, and have been acquired within the Molluscan phylum. Examined carefully they are found to differ as fundamentally in every detail from those of the Vertebrate as does the whole organisation of the Mollusc differ from that of the Vertebrate in general.

Resemblances between the Arthropod and the Vertebrate are not so striking; when they do occur they can be shown to be of the same nature. Here also the various organs which acquire some likeness to each other in the two groups are found to differ as fundamentally in detail as they do in origin. What the two groups really have in common is only that which they have both inherited from a very early undifferentiated ancestral stock.

Dr. H. GADOW, F.R.S. (Visitor), followed, and said:—When Dr. Gaskell explained his hypothesis at a meeting of the Cambridge Philosophical Society, fourteen years ago, I was the only one who had the courage of pleading for its being given a chance. It has survived pitiful contempt and ridicule.

If we want to join the ends of a broken chain, we must be clear about the links. I propose pointing out the last Vertebrate link, by reconstructing an early Vertebrate analytically.

Ever since Gegenbaur based his investigations into the composition of the cranium upon Elasmobranchs, and as since, after him, Balfour discovered so many important features in their embryonic development, the Elasmobranchs have come to be looked upon as the ideally lowest typical Vertebrates. Dohrn even went so far as to explain the Cyclostomes out of the way of direct ancestry as degenerated Elasmobranchs.

This Elasmobranch worship is wrong. They are a side-branch which leads to nothing. The main stem of the Vertebrate descent passes through what we may call Gano-Dipnoi, and their ancestors, Proto-Gano-Dipnoi, presumably were still devoid of paired limbs, and still lower down were not yet Gnathostomes. We can reconstruct further: With a mouth not terminal but ventral: their bulk consisting of a large anterior complex and a short, tapering tail, both segmented and metameric. Condensation and fusion produced a head which was so large because it contained all the principal organic systems, as nervous, digestive, respiratory, vascular, and possibly excretory and generative.

Metamerism in this anterior complex, the incipient head, was doomed, but in the posterior portion it underwent renewed activity. Not only were more segments formed by interstitial budding, but metamerism ran wild, culminating, besides other features, in vertebralization.

The latter proceeded from the tail end forwards, and it is idle to seek for vertebræ in the primitive head, excepting in the part from the vagus backwards, which in the early creature we are dealing with, was a very recent formation.

Meanwhile, the posterior or tail portion becoming larger, part of it, from before backwards, was converted into a trunk, as this was receiving most of those organs which were crowded out from the consolidating head, and also no doubt owing to the repetitional budding backwards of some of these organs. Thus we have arrived at a Tadpole-shaped Vertebrate of which some Ostracodermi with their vertebralized tails are not a bad sample.

Gegenbaur had taught us to consider the spinal cord as an outgrowth from the older brain. The greater part of the chorda is likewise due to a secondary growth backwards, this organ not being laid down in its totality, certainly not in the tail where it ought to have arisen if originally intended for an axial stiffening organ. It arises, however, in the trunk, and since this is a later



addition (due to interstitial postcephalic budding) the chorda must be of a comparatively late stage.

Both these features, chorda and spinal cord, fit into the sketch I have just outlined, but if we consider the spinal cord as an outgrowth from, and therefore a thing later than, the brain, this seems to go strongly against Dr. Gaskell's theory, and this would not be reconcilable with my early Vertebrate. But Gegenbaur's explanation, development from the supra-oesophageal ganglia of the Invertebrates, is one of those captivating notions which is really nothing but a working hypothesis to account for the dorsal position of the spinal cord. And yet this hypothesis, absolutely wrong in detail, led and became wrapped up in the much more important principle of the foundation of a trunk by backward interstitial budding. As this became dimly recognised as reasonable, the spinal cord explanation benefited by it, although wrongly.

A few words about the skeletal material, the cartilage. I remember Gegenbaur saying in his lectures, "Aller Knorpel kommt ursprünglich von Aussen." We are only now beginning fully to understand the meaning of that oracular sentence. The cartilage of the Vertebrata is originally an ectodermal, basal membrane product, which then migrates inwards. It does not arise, as the old master himself had taught, and as everybody teaches, in the immediate vicinity of the chorda, there to form arcualia or basal blocks, these to form neural and ventral processes, whence ultimately arise the median fin-supporting rays. The process is just the reverse. First rays, lastly basal blocks, culminating in the formation of an axial skeleton with centra. As an aside, I need scarcely mention that this reversed process considerably assists the derivation of the paired fins from a hypothetical lateral fin.

Another point: since Gegenbaur has stated it positively, there have been persistent attempts to prove that cartilage appears endogenous in the chorda. Personally I think that this belief rests upon faulty, or misinterpreted observations, but if there should, after all, exist such endogenous chordal cartilage, such an endodermal origin would appear quite irreconcilable with the new doctrine of its ectodermal origin. And yet, if Gaskell's explanation of the chorda as an early folded-off portion of his new gut is right, then it becomes quite comprehensible how this new gut-wall may still retain some lingering scleroblastic cells, since, according to Gaskell, this gut is partly made out of ventral ectoderm.

The early Vertebrate I have just reconstructed approaches the Silurian limbless Ostracoderms. *Pterichthys* may be a belated offshoot, still retaining a pair of Invertebrate limblike appendages. Ostracoderms I hold to be the lowest known Vertebrates, not yet Gnathostomes, whether we call them Hypostomes or Agnatha, or even Cyclostomes in a wider sense.

It is one of Dr. Gaskell's happiest feats to have shown that

Cyclostomes closely resemble such Ostracoderms, not, however, as their descendants, but rather as their ancestors, although modified and even somewhat degenerated. To appreciate this, however, we must cease gazing at the Lamprey. The *Ammocetes* larva is the key. Of course, even this is not primitive enough for the earliest Vertebrate. To reconstruct this we have to take away its trunk, and such a creature may well be expected to have lived in early Silurian times. Although there is not yet known a single fossil Cyclostome from the Silurian slates to recent river-mud, such creatures may come to light and they would not be more puzzling than *Palæospondylus*.

Thus far it is plain sailing. The Vertebrate end of the broken chain is clear enough. The attempts to bring *Amphioxus* into line have not been successful, and the claims of the other "Chordata" restrict themselves to a few features of doubtful value. Nowhere could these comparisons be driven home, and what do these attempts amount to against Dr. Gaskell's detailed, almost too minute comparisons of a dozen of the most important organs? If his results were, every one, nothing but coincidences, analogies, such a state of things would be much more astonishing and unlikely than his whole hypothesis. His explanations of the huge cavities in the brain, the peculiar structure of its roof, the ventral and the neurenteric canal, are the only plausible ones ever offered. It is a somewhat forgotten fact that in some Birds there is no proper neurenteric canal, while in other species there are, not one, but two and even three successively formed communications of the central canal with the gut and passing right through the chorda. An organ which, like the chorda of a bird, has passed its prime, is liable in its degeneration to revert to primitive features, somewhat muddled. Here we have three neurenteric connections, their respective funnels behaving as if the chorda were a negligible quantity, or rather part of the gut.

Gaskell's explanation of the chorda is by far the best we have. If considered as a product of an endodermal gut, the chorda cannot well have started as a supporting organ. It must have started with gut-like function, but having lost this with its lumen, its walls shrinking to rod-shape, may then well have formed a useful axial support. Can it be upheld, that the chordoids of *Balanoglossus* and *Rhabdopleura* ever had a gut function? This would mean that a glandular, secretive organ has lost its function and yet waxed large. A proper chorda is not a glandular thing, and even when it is a rod ten feet long and more than one inch thick, it possesses neither nerves nor blood-vessels.

Zoologists have allowed histology to slip out of their hands into those of the physiologists, and it has done well there. Embryology would likewise have fared better if the function of the aggregating and growing masses of cells had been taken as the leading principle, instead of the structures which they ultimately give rise to. It is function which determines the organ, and the

possible function often depends upon mere position, sometimes almost accidental, with regard to the surrounding medium. Nature does not care where the material comes from, provided it be suitable. There are, for instance, endodermal and ectodermal gills; nor does it matter whether a creature digests and absorbs food by its inner or by its outer surface. Whilst an armour-clad animal cannot do it, a naked Amphibian drinks through its whole skin; and most species of *Spelerpes*, sometimes six and more inches long, never possess gills, have lost their lungs, and breathe almost entirely through their outer surface.

The Germ-layer theory has crystallized into a dogma and has led into a *cul-de-sac*. How else could it happen that people, who have spent a lifetime at embryology, throw it up in disappointment and denounce the theory of Recapitulation, which is about the only valuable, really priceless generalization of this direct means of studying evolution.

Because Dr. Gaskell is a good physiologist, he saw that the central canal and the brain ventricles could not possibly have anything to do, primarily, with the central nervous system, that, in fact, they are the remnants of a gut. This has of course been suggested long ago, from the general look of the thing, but there were no proofs, and there was the seemingly hopeless task of having to account for a new gut. Gaskell had the courage and insight to show how such a gut may have been evolved, and this is one of the best, simplest and most convincing chapters. It is a false dogma that the gut must be *the* organ which is homologous in all gut-possessing animals.

Dr. Gaskell may be mistaken in some of his interpretations, but his hypothesis is not only at least as good as others, but it is the only one which endeavours to carry through a great number of comparisons. His hypothesis is logically conceived; it is built up of items, none of which are impossible, therefore the total is possible.

The discussion was then adjourned to the next General Meeting, Thursday, 3rd February, 1910, at 8.0 P.M.

February 3rd, 1910.

Dr. D. H. Scott, M.A., F.R.S., President, in the Chair.

The Minutes of the General Meeting of the 20th January, 1910, were read and confirmed.

Dr. Henry Drinkwater, M.D. (Edin.), and Mr. Cyril Crossland, M.A. (Cantab.), B.Sc. (Lond.), were elected Fellows.

The discussion upon the Origin of the Vertebrates, begun at the previous Meeting, was resumed.

The discussion was continued by Dr. A. SMITH WOODWARD, F.R.S., F.L.S., who remarked that Palæontology affords no clue to the ancestry of the Vertebrates, because they seem to have originated as animals with no hard parts capable of fossilization. When they first acquired a calcified skeleton in the Upper Silurian period, they were represented not only by very primitive types like the Ostracoderms, but by true fishes of at least as high a grade as the Elasmobranchs (Acanthodians).

It is perhaps a significant fact that the Arthropods were the dominant type of life at the time when the Vertebrates began to be conspicuous. It is known that during the subsequent course of evolution of the Vertebrates themselves, each successively higher great group became the dominant type for the time being; and that each advance was due to evolution from the immediately preceding dominant type. In every case, however, the higher group seems to have been directly derived from the earliest and most generalized members of the preceding group, not from the specialized members that flourished at the time of its dominance. If, therefore, the Vertebrates originated from Arthropods, their direct ancestors must have been early generalized forms which there is little hope of discovering among fossils.

Although so little is known of their organisation, it seems probable that the Ostracoderms are lower in rank than the true fishes, and most nearly related, among surviving animals, to the Marsipobranchs. Dr. Gaskell has added to this probability by his researches on the Ammocete. His comparison of the structure of the dermal head-shield in the Upper Silurian *Auchenaspis* with that of the more deeply seated plate of muco-cartilage in the Ammocete, is particularly striking and interesting.

Most of the Ostracoderms have a remarkable superficial resemblance to the contemporaneous Arthropods of the Eurypterid group, being adapted for a similar mode of life on the sea-bottom. A few, however, are laterally compressed and as gracefully fusiform as swiftly-swimming fishes (e. g., *Birkenia*); and that these had a wide geographical distribution in Upper Silurian times is shown by the recent discovery of a fragment (named *Ctenopleuron nerepisense* by G. F. Matthew) in New Brunswick.

The supposed discoveries in Ostracoderms of appendages comparable with those of Arthropods, are due entirely to faulty observation or misinterpretation. There is nothing more than a normal branchial chamber on each side of the cranial region in genera such as *Cephalaspis*, *Pteraspis*, *Cyathaspis*, and *Tremataspis*, where the skeleton can be well observed. The so-called paired appendages ascribed to the trunk of *Cephalaspis* by Prof. W. Patten, are merely the scales which project along its sharp angulation on each side.



Prof. ARTHUR DENDY, F.R.S., Sec.L.S., contributed the following remarks:—

Any theory of the origin of Vertebrates must stand or fall by the results of detailed criticism of the evidence upon which it rests, and owing to the large amount of evidence which Dr. Gaskell has brought forward, this must necessarily be a very laborious undertaking. The portion of this evidence to which I wish to call special attention on this occasion is that which concerns the eyes, upon which very great stress has been laid. This applies especially to the median eyes, concerning which Dr. Gaskell himself states\* that “undoubtedly, in recent times, the most important clue to the ancestry of Vertebrates has been given by the discovery that the so-called pineal gland in the Vertebrate brain is all that remains of a pair of median or pineal eyes, the existence of which is manifest in the earliest Vertebrates.” This being so, it seems especially desirable to examine critically the evidence brought forward in this case. Dr. Gaskell has studied these organs in the Ammocete larva of *Petromyzon*. I myself have studied them in the *Velasia* stage of the New Zealand Lamprey, *Geotria*, which is very closely related to *Petromyzon*, and also in *Sphenodon*, where they are exceptionally well developed. I may say at once that my interpretation of their structure does not agree with that of Dr. Gaskell.

Dr. Gaskell reminds us that Crustaceans and Arachnids, as well as Vertebrates, have lateral and median eyes and that in these Arthropods, “the median eyes are in all cases eyes with a simple upright retina and a simple cuticular lens, while the retina of the lateral eyes is compound or may be inverted, according as the animal in question possesses crustacean or arachnid affinities.” Again he says, “The lateral eye of the vertebrate, possessing, as it does, an inverted compound retina, indicates that the vertebrate arose from a stock which was neither arachnid nor crustacean, but gave rise to both groups—in fact, was a member of the great palaeostracan group.” He then proceeds to examine the evidence with regard to the median eyes of *Ammocetes*, with a view to discovering whether they belong to the same type as those of Arachnids and Crustacea. He compares an extremely diagrammatic figure of the pineal eye of *Ammocetes*, which in my opinion is far from being correct, with an apparently equally diagrammatic figure of an *Acilius* larva, which, to judge from the drawing of this eye copied from Patten on a later page, is also far from accurate. By this procrustean method of treatment the two eyes are certainly made to look very like one another, although it has been impossible to eliminate the cuticular lens of *Acilius*, which is entirely wanting in *Ammocetes*.

The manner in which it has been necessary to treat the evidence in order to arrive at this comparison is clearly illustrated by

\* ‘The Origin of Vertebrates’: Longmans, Green, & Co., 1908, p. 74.

Dr. Gaskell's discussion of the minute structure of the retina. If the comparison is to be valid the retina of the pineal eye must be a simple retina, that is to say, it must not contain an optic ganglion. Dr. Gaskell says "neither I myself nor Studnička have been able to see any definite groups of cells between the nerve end-cells and the optic nerve such as a compound retina necessitates." It is difficult to reconcile this statement with what Studnička himself says. According to this author,\* the retina of a developed *Ammocete* consists of the following cell-layers:—

- (1) At the bottom, a layer of nerve-fibres, which are in direct connection with those of the pineal nerve.
- (2) A layer of basal cells; large, very clear cells with lightly staining protoplasm and large nuclei, with a number of nerve-fibres running between them.
- (3) A layer of nuclei belonging to small cells.
- (4) A layer of cylindrical cells which correspond to the rods of older authors and which consist of sense-cells and supporting cells.

This does not sound very much like a simple retina. Dr. Gaskell quotes Studnička as saying that the nerve end-cells pass directly into the nerve, which, Dr. Gaskell observes, "points directly to the conclusion that this retina is a simple, not a compound retina, and that it therefore in this respect agrees with the retina of all median eyes." I do not know where Studnička makes the statement upon which Dr. Gaskell bases this conclusion. What I do find him saying (*op. cit.* p. 25) is that the lower extremity of the sense-cell is produced into a nerve-fibre which loses itself in the nerve-fibre layer of the retina (1). He further expressly states that in the adult *Petromyzon* there are amongst the round basal cells many which undoubtedly have the character of ganglion cells, and that the processes of these cells may be followed into the layer of nerve-fibres, while they also send processes into the layer of cylindrical cells.

In short the retina of the pineal eye of *Ammocetes* is undoubtedly a compound retina and not, as Dr. Gaskell would have it, a simple one. My own observations on the pineal eye of *Geotria* fully confirm this view. In this animal also a well-developed retinal ganglion is present. Dr. Gaskell endeavours to harmonize my observations with his theory by supposing that the cells of which this retinal ganglion is composed "do not represent the original optic ganglion of a compound retina, but rather the subsequent invasion, by way of the pineal nerve, of ganglion cells belonging to a portion of the brain." When undoubted facts have to be ignored or explained away in this manner in order to

\* "Die Parietalorgane" (in Oppel's 'Lehrbuch der vergleichenden mikroskopischen Anatomie der Wirbelthiere'), p. 24.

support a theory it looks as if that theory must stand upon a somewhat shaky foundation.

Dr. Gaskell, then, concludes that in the pineal eye of *Ammocetes* "there is certainly no appearance in the least resembling a compound retina such as is seen in the vertebrate or crustacean lateral eye." It is true that in the Lampreys the retinal ganglion of the pineal eye is not spread out to form a layer of such uniform thickness as in the lateral eye, but the pineal eyes of *Sphenodon* and of the Lacertilia make a much closer approach to the lateral eyes in this respect.

By far the most important evidence afforded by both the pineal and lateral eyes of Vertebrates, however, is, in my opinion, that derived from their development. Both differ essentially from any Invertebrate eye in being formed as diverticula of a hollow brain. The eyes of Arthropods are formed by thickening and differentiation of the superficial epiblast. How is it possible to reconcile this discrepancy? Dr. Gaskell himself (*op. cit.* p. 101) states the problem quite clearly in the case of the lateral eyes. Having arrived at the conclusion that the retina is in this case a compound retina, composed of a retina and retinal ganglion of the type found in Arthropods, he goes on to say: "From this it follows that the development of the vertebrate retina ought to show the formation of (1) an optic plate formed from the peripheral epidermis and not from the brain; (2) a part of the brain closely attached to this optic plate forming the retinal ganglion, which remains at the surface when the rest of the optic ganglion withdraws: (3) an optic nerve formed in consequence of this withdrawal, as the connection between the retinal and cerebral parts of the optic ganglion." Of course, the same must apply to the pineal eyes\*.

Relying upon Götte's observation "that the retina arises from an optic plate, being the optical portion of his 'Sinnesplatte,'" Gaskell concludes that the retina (of the lateral eye) is to be regarded as a portion of the superficial epiblast together with a retinal ganglion with which it has become fused, while the optic vesicles are explained as outgrowths of the primitive Arthropod stomach which supply only the epithelial and supporting framework of the retina, with which the nervous and sensory elements become interwoven. The development of the lateral Vertebrate eye is, however, a very complex process, and as I have not made a special study of it myself, I leave it on one side, though I may say that Dr. Gaskell's idea of the double origin of the retina and its supporting structures seems to me to be too far-fetched to be of much value as a support for his theory, and that any attempt to institute a close comparison between the lateral eye of a Vertebrate and the highly specialized compound eye of an Arthropod is foredoomed to failure.

\* At any rate so far as no. (1) is concerned, whatever view we may take as to the presence or absence of a retinal ganglion in the pineal eye.

Dr. Gaskell unfortunately does not deal with the development of the pineal eye, which is far simpler. This has been carefully studied in various types, all of which agree in essential features. I myself have studied it chiefly in *Sphenodon*, upon which animal the following statements are based. The pineal eye originates as a simple evagination of the brain-roof. This completely separates from the brain and closes up. The optic vesicle thus formed does not invaginate to form an optic cup, as in the case of the paired eye, but the retina, with its sense-cells, ganglion-cells and nerve-fibres, is formed directly and *in situ* by differentiation of its posterior wall, while the lens is formed from its anterior wall. There is not the slightest indication of the origin of any part of the retina directly from the superficial epiblast. It is true, of course, that the whole of the central nervous system is derived, in the first instance, from superficial epiblast, and so also is the central nervous system of an Arthropod. No one denies that the retina is epiblastic in origin; the question is, what part of the epiblast is it derived from? In the Vertebrate it is derived from the part which becomes invaginated to form the central nervous system. In the Arthropod and in other Invertebrates, it is not.

I cannot, therefore, avoid expressing the opinion that the evidence which Dr. Gaskell derives from the study of the lateral and pineal eyes in favour of his theory does not stand the test of critical examination. It appears to me, if I may venture to say so, that he has failed to distinguish between analogy and homology. Animals which have to live under similar conditions must be expected to become adapted along similar lines, and it is no more necessary to invoke a common ancestry to explain the resemblance between the visual organs of Vertebrates and Arthropods than it is to give the same explanation of the superficial resemblance between their organs of locomotion. Again, the resemblance between the lateral eyes of Vertebrates and the highly characteristic compound eyes of any Arthropod is not nearly so striking as is that between the former and the higher Cephalopod eye, and yet no one, so far as I am aware, has yet ventured to include the Octopus in the ancestral portrait gallery of the Vertebrata.

Looking at the problem for a moment from a wider point of view, I should like to express my agreement with those who see in *Amphioxus* a close approximation to the starting-point of the great Vertebrate phylum. The evidence in favour of the essentially primitive character of *Amphioxus* is, to my mind, overwhelming, but the acceptance of this evidence is fatal to Dr. Gaskell's views, for in *Amphioxus*, of course, a very large proportion of the Vertebrate characters upon which he lays so much stress as indicating Arthropod affinities, have not yet put in an appearance. Thus, for example, there is no trace of either lateral or pineal eyes, and we therefore conclude with confidence that



these structures have not been inherited from any Invertebrate ancestor at all, but have arisen quite independently within the Vertebrate group.

In connection with Dr. Gaskell's theory, the question is sometimes asked:—If the cavity of the central nervous system of the Vertebrate, with its lining epithelium, has not been derived from the alimentary canal of an Arthropod ancestor, how do you account for its existence, and how do you account for the existence of the choroid plexuses? To the zoologist, of course, this question presents no difficulty. One of the commonest phenomena of development throughout the Animal Kingdom is the increase of surface by the formation of folds. We are familiar with it in glandular tissues and in respiratory tissues, and we are familiar with it also in the formation of the central nervous system of various Invertebrates, as Professor MacBride has already pointed out. No one doubts, moreover, that this is the explanation of the convolutions of the brain in higher Vertebrates. Why then object to apply the same principle in explanation of the origin of the Vertebrate nervous system by invagination of the superficial epiblast? The Vertebrates inherited from their Invertebrate, worm-like ancestors, this characteristic mode of forming the central nervous system, which naturally resulted in the development of a hollow tube with at first a narrow lumen. Further evolution of the nervous system was brought about primarily by the increase in number of the nerve-cells and the consequent thickening of the wall of the neural tube. It will, of course, be asked by the supporters of Dr. Gaskell's theory, why has the cavity of the original neural tube increased to such enormous dimensions in the case of the ventricles of the brain? Here again I do not see any difficulty. The great mass of nerve tissue formed in the brain requires some very well developed system for nutrition and respiration. This is primarily effected of course by the cerebral blood-vessels; but we have also the cerebro-spinal fluid, with which the ventricles of the brain and the *canalis centralis* of the spinal cord are filled, and which probably exercises an important respiratory and possibly also other functions. I suppose Dr. Gaskell will hardly ask us to look upon the cerebro-spinal fluid as representing the digestive juices which were poured into the stomach of the ancestral Arthropod.

What about the choroid plexuses, then? Here, again, we have a beautiful illustration of the principle of folding in order to increase surface, a folding which is quite inexplicable except on the assumption that the choroid plexuses fulfil some very important function in connection with the cerebro-spinal fluid into which they dip. They are, as everyone knows, extraordinarily vascular (which the wall of the Arthropod stomach is not), and they probably constitute a kind of intra-cerebral gills concerned in the respiration of the cerebro-spinal fluid; they may also have other functions in connection with this important fluid.

It appears from Dr. Gaskell's opening speech that he assumes that the anterior opening of the neural tube in the larval *Amphioxus* represents the old Arthropod mouth, but in the higher Vertebrates he locates this ancestral mouth in the region of the infundibulum. This necessitates the supposition that the anterior neuropore is identical in position with the infundibulum, a supposition which would, I imagine, strike modern embryologists with amazement.

Then again, what is the value of the evidence afforded by the so-called neurenteric canal? This structure, if structure it can be called, simply results from the fortuitous enclosure of the blastopore by the uprising neural folds, and to my mind it has no phylogenetic significance of the kind attributed to it by Dr. Gaskell.

It was urged, I think by Professor Starling, that the immense physiological importance of the central nervous system gives it a special claim to consideration as evidence in the discussion of the origin of Vertebrates. This is entirely contrary to the usually accepted views of systematic zoologists, who find in structures which are apparently of the least use to their possessors\* the best guides to genetic affinity. Organs which are of great use must be subject to adaptive modification in accordance with the changing needs of the organism. Modern schemes of classification are indeed largely based upon this principle, and certain modifications in the nervous system of tape-worms have been explicitly ruled out as guides to classification in accordance therewith.

[The central nervous system of a Vertebrate of course agrees with that of an Arthropod in exhibiting traces of a fundamental metamerism, because both Vertebrates and Arthropods are metamERICALLY segmented animals, and both have very probably been derived from some metamERICALLY segmented common ancestor.

It is the later modifications, cœnogenetic rather than palingogenetic features, readily explicable as adaptations to the special needs of the Vertebrate organisation (which are of course in many respects similar to those of the Arthropod organisation), that I consider to be inadmissible as evidence in considering the phylogenetic relationships of the Vertebrates. The fact that highly specialized characters of the brain may afford a useful clue to relationship *within the limits of the Vertebrate phylum* does not, in my opinion, affect the question at issue. In dealing with closely related groups comparatively recent modifications are of undoubted taxonomic value; but in comparing such widely divergent groups as Vertebrates and Arthropods, resemblances due to such characters, when they can be explained quite reasonably as the result of convergent evolution, must be eliminated from the discussion.]

\* I may cite in illustration the microscleres or so-called flesh-spicules of siliceous sponges, with their extraordinarily diverse and apparently specifically constant modifications.

SIR RAY LANKESTER, F.R.S., F.L.S., said he was not prepared there and then to discuss points of detail, but the subject was so interesting that he should wish to offer some remarks. Moreover he gathered from Dr. Gaskell's book, and from more direct information, that he himself was to some extent connected with the genesis of Dr. Gaskell's views, since certain observations and arguments of his own on *Limulus* and the Scorpion had germinated in Dr. Gaskell's mind and led him to the very careful and elaborate studies which he had made and the extraordinary theory which he advanced. Whilst calling it an "extraordinary" theory, he did not wish it to be supposed that on that account he wished to reject it or not to give it full attention. This was a matter not to be treated as *à priori* impossible or improbable, but the question simply was, "Are the facts brought forward by Dr. Gaskell such as to make it appear probable that the Vertebrates have developed from Arthropods resembling *Limulus* by the conversion of the old alimentary canal into the neural tube and the simultaneous formation of a totally new digestive tract?"

The relations of animal forms to one another is the great problem of morphology. A hundred and twenty years ago morphologists still believed in the "scala naturæ" and a linear progressive series of animal groups. The great step was taken by Cuvier in opposition to the conception of Lamarck of arranging animal forms in four branches—"embranchemens" he termed them, the Radiata, Mollusca, Articulata, and Vertebrata. He thereby anticipated the modern conception of a branching pedigree, which became the generally accepted form of classification when once Darwin had established the theory of Descent.

The earlier attempts at a branching pedigree made by Haeckel differed from the later ones by the same naturalist, and there had been considerable development and improvement in the theoretical pedigree, which aimed at exhibiting the genetic affinities of all animal forms. The question of the position of the Tunicata had been one of the most interesting. Allman, forty or more years ago, considered the Tunicata as related together with the Polyzoa to the Lamellibranchs and other Mollusca. He regarded the perforated pharynx of the Ascidian as formed by the fusion of the gill-plates of a Lamellibranch along their free edges to form a closed sac, and this was perhaps the largest call upon the imagination which had been made by a modern morphologist until Dr. Gaskell suggested the conversion of the Arthropod's digestive tract into the spinal cord and the formation of a new gut in Vertebrata by the closing in of an open ventral groove. The facts brought forward by Kowalewsky had determined the position of Ascidians in the Vertebrate stem. There were four "coincidences" of structure which by the law of probability led to the conclusion that Ascidians were genetically closely related to Vertebrata. They were the existence in the Ascidian tadpole as well as in Vertebrata (1) of the notochord developed from endoderm,

(2) of the pharyngeal gill-slits, (3) of the tubular dorsally placed nerve-cord, and (4) of the cerebral eye. The evidence was cumulative, and its value depended on the exact and indisputable nature of the agreements and on the fact that they were found in the two cases compared and in no other animals, so that a common inheritance of these structures by Ascidians and certain Vertebrata, not shared by other forms, was the only rational explanation of the facts. Was this the case with the coincidences of structure between the Lamprey and the Arthropods brought forward by Dr. Gaskell? Sir Ray Lankester held that the coincidences cited by Dr. Gaskell were not of a sufficiently exact and special nature, nor peculiar to the Vertebrates and Arthropods, so as to render it necessary to suppose that Vertebrates had been derived from Arthropods, and certainly not of such a nature as to render it reasonable to suppose that the extraordinary conversion of the Arthropod's digestive tract into the nerve-tube had taken place as insisted upon by Dr. Gaskell.

The view which was almost universally accepted at present by zoologists was that when once we pass from the Cœlenterate or Entero-cœlous grade of animal structure to the Celomata or Celomo-cœlous grade, a number of diverging great lines of descent or phyla must be recognised—such as the Echinodermata, the Appendiculata (including Arthropods, Rotifers, and Annelids), the Mollusca, the Vertebrata, the Nemertina, and other worm-phyla. As to the beginnings of any of these lines of descent, we had (as was natural enough) very scant indications, nor could we say anything as to the early connection of any one of these great phyla with another. What appeared highly probable, if not certain, was that they all converged to simpler ancestral forms, and that they all inherited the same fundamental tissues, digestive tract and glands, nephridia, cœlom and cœlomic ducts, reproductive gonads, blood-vascular system, and nervous cords (many or few), and essentially the same types of sense-organs—ophthalmic, auditory, gustatory, olfactive, and tactile. That the optic vesicles of Arthropoda should agree, not absolutely but in many important respects, with those of Vertebrata, could not be held to indicate special affinities since Annelids, Molluscs, and even Echinoderms had organs of the same kind. That some of the tissues should agree minutely in two of the phyla was not suggestive of special affinity, since many of the tissues agreed in most of the larger phyla. Sir Ray Lankester held and he desired to state it without any offence, that in searching by long and strenuous enquiry for evidence in favour of such a hypothesis as that adopted by Dr. Gaskell, the mind is liable to a kind of "suggestion," and that the psychological condition may become similar to that of those who too readily admit all sorts of coincidences as evidence that Bacon wrote the plays of Shakespeare. The heroic nature of the task which it is sought to accomplish undoubtedly in many enterprising and devoted investigators has re-acted unfavourably on the



judgment. All are liable to it and it may be that something of the kind is here at work. Though he could not follow Dr. Gaskell in the theory put forward by him as to the origin of Vertebrates, he recognised very gratefully the value of the observations on many details of structure to which it had led that distinguished physiologist, and also the new observations which it had called forth on the part of other naturalists, such as the interesting additions to our knowledge of the head-shield and the body-scales of *Cephalaspis* which had just been placed before the meeting by Dr. Smith Woodward. He thought the Society was to be congratulated on a very interesting debate. (In the further course of the discussion Sir Ray Lankester stated that whilst he considered *Amphioxus* and the Ascidian tadpole to present in many points of structure a very much more primitive phase of the Vertebrate group than do either Lampreys or Fishes, he held that they were also specially modified and degenerate each in its own way, and were not closely representative of the main line of descent. He considered that the remains of the earliest known fossil fishes, on account of their necessarily incomplete condition, were not capable of throwing much light on the question of Vertebrate ancestry. He was led to the conclusion that *Balanoglossus* threw some light on the subject, and he drew attention to the remarkably complex brain and cerebral respiratory pits of the Nemertine worms and the dorsal median as well as lateral nerve-cords of those creatures, which had led Hübner long ago to suggest their close connection with the remote ancestors of Vertebrates. A large survey of the facts of animal structure, even including that of unfamiliar marine worms, was necessary in order to form a reasonable judgment on the question of Vertebrate ancestry.)

Dr. P. CHALMERS MITCHELL, F.R.S., F.L.S., remarked that consideration of the general morphology of the nervous system enables us to place the Vertebrates in their true perspective amongst the various Invertebrate groups. In the Cœlentera, as shown by the Hertwigs, the nervous system frequently appears as a diffuse layer of cells and fibres underlying, and in close connection with, the epidermis, whilst there is much evidence that a similar primitive condition underlies the various presentations of the nervous system in higher groups. Even amongst the Cœlentera, two processes coincidently or independently result in modification of the primitive simplicity. The original diffuse layer may become thickened in definite regions, forming, for instance, rings round apertures or radial bands, whilst in the intervening areas it may be obliterated. The thickened bands or rings may migrate inwards and lose their intimate connection with the epidermis. Similar processes varying in position and extent of their incidence have led to many different arrangements of the nervous system in the higher groups.

In the Turbellaria, inward migration has taken place, and two ventro-lateral cords have been formed.

In the Trematodes, inward migration has taken place, and there are six cords, two dorsal, two ventral, and two lateral, with a network of connecting cords, some of which form a series of hoop-like rings.

In the Cestodes there is less inward migration, whilst there are two lateral cords with occasional transverse connections.

In the Nemertines, sometimes there is no inward migration, so that the nerve-strands remain strictly sub-epidermal; sometimes the strands have completely separated. The primitive continuous sheath is frequently retained with two lateral and sometimes one dorsal thickening.

In the Nematodes also the extent to which inward migration has taken place varies very much, in some cases the sub-epidermal position being retained. Six strands occur in many forms, one dorsal, one ventral, and two at each side; these are connected by traces of the primitive continuous sheath in the form of a very broad anterior hoop, and narrow posterior strands. A different arrangement of these antero-posterior strands occurs in front of the nerve-collar.

In *Gordius*, inward migration has occurred and there are three ventral strands.

In Arthropods, the inward migration and separation from the epidermis are complete, and there are two ventral bands with an anteriorly placed collar.

In *Balanoglossus*, there is a continuous sub-epidermal sheath which has not migrated inwards, and special dorsal and ventral thickenings, and also in the collar region the very interesting short neural tube with anterior and posterior neuropore formed by invagination.

In Chordates, there is a single dorsal band which migrates inwards, whilst the outgrowing segmental nerves may be taken as specialized representatives of the continuous sheath.

From the point of view of the general morphology of the nervous system, therefore, the Chordate or Vertebrate group exhibits simply one of a large series of different modes of specialization of the primitive diffuse, sub-epidermal sheath.

In quite a number of these different experiments, the processes of segmentation and of cephalization with the formation of a brain have occurred independently, and have produced analogical or homoplastic structures. The elaborate comparison of the results of the processes of cephalization and segmentation in *Ammocetes* and higher Vertebrates with those of the Arthropods are meaningless unless we suppose that *Amphioxus* has passed through such a stage and has lost all traces of it; it is a simpler supposition that the higher Vertebrates have independently acquired the results of cephalization after having passed through a stage of which *Amphioxus* is the nearest living although specialized and degenerate representative.

As Prof. Gaskell has laid so much stress on comparison between

the brain and central nervous system of Arthropods and Vertebrates, it is interesting to notice that C. Judson Herrick, another distinguished physiologist and psychologist, has recently compared the two sets of organs (Address of the Chairman of the Section Zoology; American Association for the Advancement of Science, 1909, printed in 'Science,' 1910, p. 7). Professor Herrick, reviewing the subject without reference to any theory of origin, comes to the conclusions that the psychological processes of Arthropods and Vertebrates differ totally; that the difference of function is correlated with a fundamental difference of type underlying all superficial resemblances, and which was "foreshadowed far back among the ancestral crawling things in which no truly vertebrate character was manifest, foreshadowed merely by a structural type with different latent potencies."

Professor STANLEY GARDINER, F.R.S., F.L.S., said:—Of the many speakers only Dr. Gaskell has put forward a connected theory which the rest have merely attempted to destroy. Their alternative plan is by a line of evolution through *Amphioxus*, but they do not attempt to show us how this beast may have been produced. Unfortunately in the whole question of the Origin of Vertebrates we have very few real facts upon which to base our views. Such facts, so far as I can see, will be obtained from the study of extinct forms, and it is a most curious fact that nowhere has Palæontology yet shown a series of transitional types between distant groups. We have to content ourselves with *conclusions from analogies* and *proofs by probabilities*. We largely study existing forms. The danger of this is well exemplified when we consider the relations of Reptiles to Mammals. Both groups as existing now must largely be traced to *Theromorphs*, of which, following Cope, minute and relatively puny forms probably branched off into each of the two phyla. Applying the ordinary terminology of Cope, it may be said that existing Reptiles have regressed and that existing Mammals have progressed. We may now consider this line as fairly well established by *analogies* and *probabilities*, and it appears to me that it is a line almost of facts to which we can appeal with considerable certainty for zoological canons. If there is one point more than another which it shows it is surely the paramount importance of considering the condition of the central nervous system a test of progression, as Dr. Gaskell maintains. It demonstrates with certainty that his deductions from the brains of living Vertebrates, as such a test, are absolutely justified. In opposition to Professor Dendy I should claim that the central nervous system is the best organ on which to trace the changes of evolution. It governs every organ in the body, and it must reflect in its own structure every change which those organs undergo, every act of progression.

Turning to Amphibia, we have no indications of their real origin, and we have still less when we come to the Fishes. The

Leptocardia and the Marsipobranchia are with no certainty represented in the fossil state. They are derived from an ancestor far more ancient than the *Theromorphs*, and any comparison of existing forms, supposed to have been derived from this ancestor, might well show vastly greater differences than between say Primates and Lacertilia, or even Primates and Pisces.

The weakest part of the MacBride-Goodrich argument the other night lies in their consideration of *Amphioxus* as a simple primitive Vertebrate. Whatever *Amphioxus* may be, it is surely not in the main stem of the Vertebrate descent, and it is certainly a very specialized form. To argue, as Goodrich did, that the presence of primitive excretory cells (solenocytes) in *Amphioxus* proves it to be primitive, and related to the Annelids, comes to the same thing as claiming that *Phoronis* is also an Annelid, because its larva has similar cells.

Examining both the above groups, and applying "every canon of Biology," we must, I conceive, regard *Amphioxus* as equally typical of regression as is any beast that exists in the Animal Kingdom, while the Marsipobranchia as typically show progression. Looking at the groups from this point of view the Leptocardia may be cast aside from our discussion as unprofitable, and we can turn with certainty to considering the morphology of Marsipobranchs for some guide to the evolution of Vertebrates.

It is not my desire to draw your attention to the series of facts, both physiological and morphological, discovered by Dr. Gaskell in his extensive comparison of the higher Invertebrates with the lower Vertebrates. They present an extraordinary series of analogies and probabilities which cannot be lightly passed over, and, even if his views be ultimately rejected by palæontological discoveries, will for ever make Zoologists indebted to him for drawing their attention to a fresh and broader aspect in which to consider their science. Of his comparisons I would particularly draw attention to that between the internal cartilaginous skeleton of *Limulus* and that of *Ammocætes*, the skeleton being a part which, judging from fossil and living Vertebrates, seems to retain for the longest period traces of all its developments, "earmarks," as Osborne terms them. I might refer also to the infundibulum, the commissures of the brain, the thyroid, the auditory apparatus, and the existence of giant fibres and cells in the nervous system. By far the simplest way to explain this extraordinary series of coincidences between the organs of different forms is to suppose that they are due to a common inheritance.

I would turn now rather to the difficulties which beset the view, and by far the chief of these must be deemed to be that relating to the alimentary canal. To get that of *Petromyzon* from that of *Ammocætes* we have an entirely new formation of quite startling character. This is a fact, and accepting it as such we can proceed with our minds more open, I think, to consider how a gut in Vertebrates came into existence. Professor MacBride is quite



Haeckelian in his views of the gastrula—or at least of the germ-layer theory, which he claims to be stronger than ever. If there is a real fundamentally important separation such as he claims between the germ layers, it is quite inconceivable that there could be formed cells of one layer from those of another layer. In regeneration of tissues we have clear evidence that ectoderm can form mesoderm and endoderm, that endoderm can form ectoderm and mesoderm. Mesoderm is not very happy in its formation of the other layers, but Dendy has shown that in *Antedon* the endoderm can come from ectoderm and from mesoderm.

I would altogether dissent from Sir Ray Lankester's line of evolution from the gastrula. I am inclined myself at present to regard the Annelids as coming from some Actinian-like ancestor. In this, as in all Actinia, the secreting digestive epithelium, that of the stomodæum and mesenterial filaments, is derived from (grows down from) the ectoderm after the whole of the gastro-vascular cavity is lined by an epithelium which is capable of ingestion but not of extracellular digestion. My own work is not sufficiently advanced perhaps for me to make this statement, but such were the indications I obtained. The lining epithelium of the cavity would be equivalent to and homologous with the endoderm of *Hydra*, and it would form the mesoderm of three-layered animals, the endoderm being an entirely new formation. I am aware that there are great, even insurmountable, difficulties in respect to this view, but the ectoderm and endoderm of higher forms appear to me to be far more intimately related in their functions than are either of them to the mesoderm.

In the experimental work of Driesch, Wilson, and others, we get into a maze of difficulties in regard to the preformationist hypothesis. Blastomeres, it is clear, are to a large degree interchangeable. Incidentally, a fourth blastomere gives a gastrula in *Amphioxus*. Again, in budding there are difficulties with this theory, the gut of some budded-off Polyzoa being formed from mesoderm, while of Tunicates, supposed relations of the Vertebrates, *Clavellina* buds from the endoderm and *Botryllus* from the ectoderm, giving ectoderm and endoderm respectively; and do not some Sponges turn inside out to give the adult?

I need scarcely go further into the question of the germ-layer theory. The confusion when it is applied to Vertebrates is obvious, and we get everywhere involved in difficulties in Invertebrates. If the gastrula be a general stage on which great stress is to be laid, it necessarily might be supposed that the stages up to it should be the same, while actually in the segmenting eggs we get the most diverse fates for the individual cells.

On the whole it is abundantly clear, it appears to me, that it is the nurture as well as the nature of the individual organs which is to be discussed. The law of recapitulation in embryology has only a limited applicability. Surely the transitory characters are at best only a very partial reminiscence of the structural types

through which the adult may be supposed to have passed during the geological ages. In all these stages the embryo has itself been subject to specialisation. I think that where Dr. Gaskell errs is in laying too much stress on many details of the recapitulation hypothesis. Some of his resemblances I can conceive might be due to convergent or adaptive evolution, acting upon lines almost infinitely long before the common ancestor is reached. Yet there remains such a mass of hard analogy, borne out too by the most careful physiological and morphological investigation, a mass which cannot be put forward—or even a tithe of it put forward—by the exponents of any other view, that one is inclined to doubt the presence of adaptive evolution at all in this case. Although I should feel it to be “non-proven,” I cannot but regard it as by far the most striking view of the origin of Vertebrates that has yet been expounded.

Morphologists must carefully consider whether they may not be holding on to shibboleths, and wilfully blinding their eyes to the great mass of facts, many largely physiological, which has in recent years been accumulated. Is it not just as necessary for the zoologist, who wishes to consider these great questions, to be a physiologist as it is for the latter to be a morphologist? If it is desired to prove Dr. Gaskell's hypothesis wrong, his points must be taken fact by fact to see where they lead—as indeed barristers do with evidence in our courts. If it is desired to prove some other theory right, it must likewise be taken fact by fact, and no one can, as some try to do at present, consider the nature of any beast without any examination into its nurture.

The Rev. T. R. R. STEBBING, F.R.S., F.L.S., said: Mr. President, may I be allowed for a few moments to intervene on behalf of those among us who may describe themselves as the know-nothing section of the audience, persons not a few who are committed to neither side in the controversy? When we return home and our friends gleefully enquire, “What then has been decided as to the Origin of Vertebrates?” so far we seem to have no reply ready, except that the disputants agreed on one single point, namely, that their opponents were all in the wrong. It occurs to me to illustrate the position by propounding another enigma. What is the origin of arguments? Take an example. Suppose a company in which some pedantic arithmetician asserts that two and three invariably make five. To those who like myself easily fall in with the views of the last speaker, the statement appears incontrovertible. But in some brains any positive declaration at once sets up what may be called an intellectual wriggle. This process soon enables the contradictory person to point out that two and three sometimes make six and sometimes minus one or plus one, as well as two-thirds of one or one and a half. Since one operation in arithmetic is as good as another, if not a great deal better, it follows that two and three do *not* invariably make five; far from it. Thus the wriggling of the brain originates argument.

Incidentally I may refer to two points raised by those who object to connecting the origin of Vertebrates with the Arthropoda. It was represented that the chitinous envelope of the latter was prohibitive of cilia. The delicate auditory cilia of Crustaceans are well known to carcinologists, but I am warned by intelligible signals that the term *cilia* is variously applied in different branches of Natural History. Another objection was founded on the difficulty of believing in the transfer of function from one organ to another, as required by Dr. Gaskell's hypothesis. But on this head the ingenuity of Nature seems to have been signally vindicated by the late Professor Gegenbaur, who showed how one part of an animal organism, in proportion as it went out of service for one function, could be appropriated for another.

Now, on the general question we have admired Dr. Smith Woodward's interesting account of the earliest fossil fishes. But these are accepted Vertebrates. For the origin of Vertebrates we must go back to something that is not a Vertebrate, such as may have existed perhaps far back in the Laurentian period. Imagine some soft, more or less elongated, animal organism wriggling about in the primeval sea. Then, as now, the hard conditions of the World demanded some sort of hardening on the part of living creatures. Some would find advantage in a stronger external coating, others in a strengthened central axis. But in either case the necessity of wriggling would often be paramount, giving rise on the one hand to a segmented exoskeleton, on the other to a jointed backbone. In these wrigglers, Mr. President, you have the origin of the Vertebrates,—a theory which it will be difficult to refute, as the supposed animals have hitherto revealed absolutely no relics.

The President having called upon Dr. Gaskell to reply, that gentleman said:—

It is impossible for me in the short time at my disposal to deal thoroughly with all the speakers in the two days' discussion. I will, however, do what I can.

Prof. MacBride in his latest paper prefers, as he said many years ago, to attribute my explanation to my diabolical ingenuity. As I have stated in my book, there is absolutely no ingenuity on my part; given the one fixed point that the infundibulum represents the old oesophagus and the animal remains upright, all the resemblances between the two groups of animals to which I have drawn attention, naturally follow. The devil is not in my ingenuity but in Nature's facts. I can sympathize with MacBride, for surely there could not be a more diabolical trick than to create from a lowly organised unsegmented animal whole groups of animals becoming more and more segmented, all characterised by the presence of an alimentary canal ventral to the nervous system, and then wipe them off the face of the earth, so that no trace of this sequence of forms is left among living animals. Not content

with this, this diabolic agency has left as the end result a segmented animal—the Vertebrate—in which with the greatest cunning he has not only made the supra-infundibular brain the exact counterpart of the supra-œsophageal and the infra-infundibular of the infra-œsophageal, but has grouped that nervous system round a large epithelial bag, which has nothing to do with a nervous system, but most ingeniously has been put in the exact position of the cephalic stomach and œsophagus of the Arthropod animal. To complete the story and give it an artistic finish, this ingenious devil plants above the brain of the Vertebrate impostor two median eyes exactly in the position of the two median eyes of the Palæostracan. He does not put them there for the purposes of sight, for they are functionless and degenerate in all Vertebrates, but just for sheer mischief, and how he must have chuckled over the happy thought of making them partly degenerate in the lowest Vertebrates, for he well knew that in the *Limulus* and his contemporaries they were already markedly degenerate and that they were no longer the chief eyes but their place had been taken by the large lateral eyes.

Prof. MacBride has asserted again, without giving instances, the statement so often made before, that I assume violent changes of function. Again and again I have denied it, and shown that I do not assume any such thing. Yet again I will go through the list of resemblances and ask where this statement of MacBride's is justified. The function and structure of the supra-infundibular brain is the same as that of the supra-œsophageal ganglia; the two lateral eyes and the two median eyes are the same in the two groups; the median nose has the same structure, the same nerves, and the same olfactory glomeruli in the two groups and even the slight difference, that the nasal tube in *Ammocoetes* opens dorsally and the olfactory passage in the Scorpion ventrally, vanishes after von Kupffer's demonstration that this *Ammocoetes* nasal tube originates as the tube of the hypophysis, which opens ventrally, and the dorsal position is due simply to the growth of the upper lip. Passing to the infra-œsophageal region, in both animals the anterior part is devoted to the organs of mastication and the posterior part to the organs of respiration; there is no change of function here, and I have given the evidence to show how the actual masticating and respiratory organs of the one group have insensibly shifted into those of the other group. Where are the violent changes of function so far? Then, if we pass into the spinal cord region, are we to look for them in the muscles or in the excretory organs or in the cœlomic cavities? Why, the whole evidence is that they are the same in the two groups; no sign of change violent or otherwise. I wonder what Prof. MacBride means. Surely this scathing indictment is not based on the thyroid gland which, I must repeat yet again, is not a mucin secreting gland but is a gland of great importance to the well being of the Vertebrate, apparently having something to do with



the manipulation of iodine. Is anything known of the function of the corresponding gland in the Scorpion? It may have the same function for aught I know. No! The only part of my theory which causes this assertion is that I have had the audacity to make a new gut and so go contrary to the laws of the germ-layer theory; I am content to leave it at that: time will show, I firmly believe, that the germ-layer theory is absolutely dead.

I come now to the observations of Goodrich; he as well as MacBride seemed, to my astonishment, to hold the view that *Amphioxus* was on the direct line of ascent to the Cyclostomes, that between *Amphioxus* and the Cyclostomes a brain had been developed with organs of sense, the eyes and nose. Surely this is a unique position! All other morphologists look upon *Amphioxus* as a degenerate animal, and degenerate in this precise direction. What conception has Goodrich of the evolutionary process, of the struggle for existence, of the survival of the fittest? Just consider it: here is a wretched animal without brains, without eyes, without a nose, victorious in the struggle for existence over the whole of the Invertebrate world. What is the driving force; how could it have taken place? Only, it seems to me, by some beneficent power taking special charge of him and assisting him in the growth of brain and of eyes and nose.

If there is one thing certain, surely it is Gegenbaur's dictum that the brain part is older than the spinal part, and further, the study of neurology shows clearly that in all animals, whether vertebrate or invertebrate, the brain is built up in connection with the optic and olfactory senses. No, the *Amphioxus* is not the ancestor of the Cyclostomes but, in my opinion, is closely related to the Cyclostomes as seen by its myomeres and the whole of the spinal region. After the Vertebrates had been well established the *Amphioxus*, in my opinion, arose by a process of degeneration from some ancestor of the Cyclostomes. Goodrich asserts that such a view is impossible, as no trace is seen in the development of the missing organs. Surely that argument is not good enough, for in the Tunicates, where a relationship with the Vertebrates is inferred from their development, such development is only found in certain members of the group and not in all.

Smith Woodward gave us an interesting discourse on certain early fishes, but I did not gather whether he thought the evidence I had brought forward pointed to the Osteostraci being Cyclostomes, though I think he favours that view. He called *Birkenia*, *Lasthenia*, and *Thelodus* Ostracoderms, and seemed to imply they were of the nature of Elasmobranchs. I cannot see why he called these scanty remains Ostracoderms, and would like to know whether, in his opinion, they were gnathostomatous, for the evidence is strongly in favour of the true Ostracoderms being cyclostomatous. In any case I see no difficulty in the presence of these forms, for surely it was likely enough that in the Upper Silurian seas some fish-like forms should have already progressed

onwards in the Elasmobranch direction with shagreen scales and possibly jaws from the primitive agnathous condition. The question of the interpretation of the lateral markings on some of the head shields of these mailed fishes is a comparatively small matter. Smith Woodward agrees with me that they show the presence of segmentation in this region, but thinks they were branchial segments; in my opinion, judging from *Ammocoetes*, they extend too far forward for branchiæ, and I think they are more likely to have been due to the presence of muscles supplied by the trigeminal nerve.

Coming to Lankester's speech I have a difficulty in finding anything to answer in it; he spoke of cryptograms and of Bacon and Shakespeare; another suggestion akin to the diabolic ingenuity of MacBride which hardly requires any further answer than I have given. He said there was no resemblance between the lateral eyes of Vertebrates and Arthropods, but that is not the point; it is not the dioptric apparatus upon which I was laying stress, but the retinal arrangements. It was the resemblance between this latter apparatus in the two groups upon which every observer from Berger to Parker has laid stress.

Finally, I come to the remarks of Dendy. He referred to the drawing of the right pineal eye of *Ammocoetes* as drawn in my book as a diagram. That is not so: the left half of the drawing is from the actual specimen, the right half is my interpretation of the meaning of the appearance seen. In my paper in the Q. J. Micr. Science all the drawings are carefully drawn by Wilson from the actual specimens and are not in any way diagrams. He referred to the finding by Studnička in the pineal eye of *Ammocoetes* of certain cells which he called ganglion cells. They are not arranged like an optic ganglion and are much more like the cells described in the median eye of *Limulus* by Lankester and Bourne, and called by them intrusive connective tissue cells. What these cells are I do not venture to assert; in any case they are present both in the median eye of *Limulus* and of *Ammocoetes*. As to *Geotria*, I have explained in my book that the cells grouped round the atrium may be nerve-cells as asserted by Dendy, but they are found along the nerve from the *ganglion habenulæ* to the eye. In the left eye of *Ammocoetes* the nerve has vanished and cells of the *ganglion habenulæ* run right into the eye. It is perfectly possible that *Geotria* represents an intermediate stage of degeneration between that of the right and left eyes of *Ammocoetes*, especially seeing that a portion of the original cavity is cut off to form the atrium by the massing of the cells in question. As to the tube of the nervous system, Dendy, as well as all the other speakers on that side, find it very convenient to leave out the infundibular prolongation in their picture of the formation of an epithelial tube, an unfortunate omission as it happens to be the main point of my argument. Dendy's view that the choroid

plexuses form a respiratory organ for the brain is novel: I wonder what Dendy's conception of a respiratory organ is.

In conclusion, I am grateful to the Linnean Society for allowing me to put my views before them, and only hope that all those who dissent from them will study for themselves organ by organ the resemblances between the two groups of animals and make up their minds whether they are accidental or, as I believe, significant of a real relationship.

The PRESIDENT said that at that hour of the evening, and after so brilliant a discussion, no one would expect a speech from a botanical Chairman, though much had been said of great general interest to all biologists. On the first evening Dr. Gaskell had directly appealed to the Chair with reference to his belief that "each higher group of animals has arisen in succession from the highest race developed up to that time." At present he would only say that the evidence on the botanical side appeared not unfavourable to such a view; perhaps he might have an opportunity of returning to this question on the 24th of May.

The President felt that any criticisms of his on the course of the discussion might well be dispensed with, for he had been anticipated by Prof. Stanley Gardiner, whose remarks appeared to him to agree, point for point, with those which would occur to the mind of any present-day botanist in listening to the discussion.

It only remained for him to ask the Fellows to return their hearty thanks to Dr. Gaskell and all who had taken part in the discussion, for the intellectual entertainment they had provided.

February 17th, 1910.

Dr. D. H. SCOTT, M.A., F.R.S., President, in the Chair, succeeded by Mr. H. W. MONCKTON, Treasurer & Vice-President.

The Minutes of the General Meeting of the 3rd February, 1910, were read and confirmed.

Mr. Henry John Jeffery, A.R.C.S., was admitted a Fellow.

Dr. Leonard Cockayne, Mr. Walter Ambrose Heath Harding, M.A. (Cantab.), and Miss Ida Margaret Hayward, were proposed as Fellows.

Mr. W. T. SAXTON, F.L.S., then gave an account of his recent investigations upon the anatomy of the genera *Widdringtonia*, Endl., and *Callitris*, Vent., of which the following is an abstract.

Evidence is brought forward in this communication to show (i) that *Widdringtonia* and *Callitris* do not conform to the "Cupressineæ" type; (ii) that *Widdringtonia* cannot be merged in the genus *Callitris*, but must rank as a distinct genus.

(i) The chief points in which these two genera differ from the Cupressineæ are as follows:—

- (a) The position of the Archegonia. In Cupressineæ these are found at the apex of the prothallus, in *Widdringtonia* and *Callitris* never at the apex.
- (b) The multinucleate prothallus cells.
- (c) The development of the proembryo. Eight free nuclei are *not* formed in these genera and the proembryo fills the archegonium.
- (d) At least three embryos may be formed from a single proembryo.

CALLITRINEÆ is suggested as a tribal name to include these two genera (possibly also *Actinostrobus* and *Tetrclinis*).

(ii) Both morphological and anatomical differences are pointed out between *Callitris* and *Widdringtonia*, which seem more than sufficient to warrant the retention of *Widdringtonia* as a separate genus.

Of the morphological differences the more important of those brought forward for the first time are:—

- (a) In *Widdringtonia* about 64 potential megaspore mother-cells are formed at the base of the nucellus. In *Callitris* about two such cells are found, half way up the nucellus.
- (b) The number and arrangement of the Archegonia differ materially in the two genera.
- (c) The microsporophyll normally bears 4 sporangia in *Widdringtonia*, 3 in *Callitris*.

Of the anatomical differences the most important is the occurrence of thickenings of the cell-wall in connection with the bordered pits in both the wood and the transfusion tracheids of *Callitris*; these are not found in *Widdringtonia*.

A discussion followed in which Prof. Farmer, Dr. Stapf, and the President engaged.

Mr. GEORGE MASEEE, F.L.S., followed with a lantern demonstration of his researches entitled:—

#### EVOLUTION OF PARASITISM IN FUNGI.

To understand clearly the evolution of parasitism it is important to grasp a fundamental point in the evolution of fungi generally. The most primitive forms were aquatic, and reproduced by zoospores which necessitated the presence of water to secure their dispersion. As the fungi gradually took possession of dry land, a second asexual or conidia form of reproduction, suitable for dispersion by wind, &c., was gradually evolved. This supplementary conidial condition is always the form that has assumed a parasitic



condition, the older sexual phase remaining as a saprophyte and developing when the host is exhausted. Parasitism is mainly the outcome of opportunity : and the fact that fungi present all stages of parasitism, and that a saprophytic fungus can be educated to become a parasite, proves that parasitism is an acquired habit. Incipient or imperfectly evolved parasites promptly kill the host, and consequently curtail the period of their own existence, as *Pythium De Baryanum*. A higher stage of parasitism is reached by many of the rusts and smuts, *Ustilago avenæ*, &c., where the host is attacked as a seedling, and is stimulated to an unusual condition of growth throughout its normal period of growth. More advanced parasites show a tendency to arrest the production of spores and conidia, and to perpetuate themselves by perennial mycelium located in some perennial vegetative portion of the host (root, tubers, &c.) or in the seed. In the most highly evolved parasites reproductive bodies are entirely arrested, and the parasite is perpetuated by hibernating mycelium only.

In the discussion which followed the speakers were Mr. H. W. Monckton, V.-P., Miss A. L. Smith, Dr. Helen Fraser, and Prof. Dendy, Mr. Massee replying.

The following papers (communicated by Prof. J. STANLEY GARDINER, M.A., F.R.S., F.L.S.) were read :—

1. "The Orneodidæ and Pterophoridæ of the Seychelles Expedition." By T. B. FLETCHER, R.N., F.E.S.
2. "Die von Herrn Hugh Scott auf den Seychellen gesammelten Embiidinen, Coniopterygiden, und Hemerobiiden." By Dr. GÜNTHER ENDERLEIN.
3. "Die Termiten der Seychellen-Region." By Dr. NILS F. HOLMGREN.
4. "On the Land and Amphibious Decapoda of Aldabra." By L. A. BOREADAILE, M.A.

March 3rd, 1910.

Mr. H. W. MONCKTON, Treasurer and Vice-President,  
in the Chair.

The Minutes of the General Meeting of the 17th February, 1910, were read and confirmed.

Mr. Hugh Scott was admitted a Fellow.

Miss Winifred Elsie Brenchley B.Sc. (Lond.), Mr. James Meikle Brown, B.Sc. (Lond. & Sheff.), and Mr. Hayward Radcliffe Darlington, M.A., LL.M. (Cantab.), were proposed as Fellows.

Mr. W. BICKERTON, F.Z.S., M.B.O.U., gave a lantern lecture

on "Our British Nesting Terns," illustrated by about 110 photographs taken by him direct from nature, of all the British species.

At the conclusion of the lecture, the Chairman, after remarking upon the interest of the subject, and the excellence of the slides, moved a vote of thanks to the Lecturer, which was carried by acclamation.

March 17th, 1910.

Dr. D. H. SCOTT, M.A., F.R.S., President, in the Chair.

The Minutes of the General Meeting of the 3rd March, 1910, were read and confirmed.

Dr. Harry Drinkwater, Mr. Frederick Hamilton Davey, and Lady Isabel Browne were admitted Fellows.

Miss Nellie Bancroft, Mr. Sidney Guest, and Mr. John Charles Wilson were proposed as Fellows.

Dr. Leonard Cockayne, Mr. Walter Ambrose Heath Harding, M.A., and Miss Ida Margaret Hayward were elected Fellows.

Dr. HARRY DRINKWATER, F.L.S., showed specimens of drawings in distemper on coloured paper, of wild-flowers growing at Wrexham; his object was to draw every plant in the local flora natural size, and he had completed 300, leaving about 500 still to be drawn.

Dr. OTTO STAFF, F.R.S., Sec.L.S., on behalf of the Director, Royal Botanic Gardens, Kew, exhibited specimens of *Eysenhardtia amorphoides*, H. B. & K., and demonstrated the exquisite fluorescence of the infusion of the wood of the plant (as described by him in the Kew 'Bulletin,' 1907, no. 7, pp. 293-305) by the aid of the electric arc-light of the optical lantern.

The Rev. T. R. R. Stebbing, F.L.S., Prof. Dendy, and Mr. Shennstone contributed additional observations.

Mr. J. H. HOLLAND, F.L.S., also on behalf of the Director of Kew, showed samples of Soy Bean, *Glycine Soja*, Sieb. & Zucc. (*G. hispida*, Maxim.), with herbarium specimens of the plant producing this seed.

He stated that the seeds of "Soy," of which there are many varieties, may be black, brown, green or greenish-yellow, yellow, or mottled; sometimes seeds are described as white, but there appears to be no Soy bean true white in colour.

The plant is variously known as "Soy," "Soja," "Soya," "White Gram," "American Coffee Berry," and "China Bean."

In China and Japan, where the plant has been cultivated for many years—perhaps centuries—the beans are an important food,

and they are also said to be used as a substitute for coffee. Bean Cake and the sauce known commercially as "Soy" is also made from them. It is stated that in the manufacture of the Soy of Commerce, in addition to the beans, the requirements are simply a large amount of salt and flour, and an unlimited supply of fresh water. Wenchow is an important centre of the manufacture, and here the bean used for the purpose is said to be chiefly the white form from Chinkiang.

The cultivation has been extended to India, Africa, and other warm countries, and in America the plant has been grown for a number of years (25 at least) as a forage crop. Like many other leguminous plants, it has a special value as a green manure.

The principal use of the beans in this country is for the extraction of the oil, of which they contain about 18 per cent. suitable for soap-making, and in general as a substitute for cotton-seed oil. The residue, after the extraction of oil, is suitable for feeding cattle, and for this purpose appears likely to become a serious competitor of cotton-seed cakes, sunflower-seed cakes, linseed cakes, &c. The beans can be bought in London at about £5 to £6 per ton; the oil realises about £21 to £22 per ton, and the cake about £6 to £7 per ton.

Beans and bean-cake exported from China have gone chiefly to Japan, and certain parts of Asia, but recently, beginning about November 1908, an important trade has been developed in them, more especially with the beans, between Manchuria and Europe, Dairen (Dalny) being the chief place of export.

The cause of this sudden development may, perhaps, be attributed to the facts that a great increase in the cultivation took place in Manchuria during the Russo-Japanese war, to meet the demands for food of the Russian Army; then, when the troops were withdrawn, the production being found profitable, and the home demand reduced, other markets were sought. The trade extended to Japan, and afterwards, assisted perhaps by a period of depression in that country, it extended to Europe, where the industry has created interest in many quarters.

The amount of the 1908 crop sent to Europe through Vladivostok up to July 1909, was 180,000 tons, the greater part destined for the English market (Hull and Liverpool), and the remainder going to German (Hamburg) and Scandinavian ports.

Up to 1907 the export of Soy beans from Manchuria did not exceed 120,000 tons annually. During 1908 the export rose to 330,000 tons (one half shipped from Dairen; 100,000 tons from Newchang, and 65,000 tons by rail *via* Suifenhō to Vladivostok), the increase it is said being due entirely to the demand from Europe. The total of the 1909 crop exported has been estimated at about 700,000 to 800,000 tons. It is anticipated that at present prices Europe may eventually take at least 1,000,000 tons annually.

Mr. Craib (Visitor), Mr. Bunzō Hayata (from Tokyo), and Dr. Stapf gave further details, and Mr. Holland replied. He

also brought for exhibition a series of 17 photographs showing the methods now used to bring Teak, *Tectona grandis*, Linn. f., from the Burma forests to the shipping ports.

Mr. E. P. Stebbing, Mr. J. S. Gamble, Mr. John Hopkinson, the Rev. T. R. R. Stebbing, and Dr. A. P. Young, joined in the discussion, and Mr. Holland briefly replied.

The following papers were read :—

1. "On the Life-history of *Chermes himalayensis* on the Spruce (*Picea Morinda*) and Silver Fir (*Abies Webbiana*) of the N.W. Himalaya." By E. P. STEBBING, F.L.S.
2. "A Contribution towards our Knowledge of the Neotropical Thysanoptera." By R. S. BAGNALL, F.L.S.

April 7th, 1910.

Dr. D. H. SCOTT, M.A., F.R.S., President, in the Chair, succeeded by Mr. H. W. MONCKTON, Treasurer & Vice-President.

The Minutes of the General Meeting of the 17th March, 1910, were read and confirmed.

Miss Winifred Elsie Brenchley, B.Sc. (Lond.), Mr. James Meikle Brown, B.Sc. (Lond. & Sheff.), and Mr. Hayward Radcliffe Darlington, M.A., LL.M. (Cantab.), were elected Fellows.

The President announced that the Linnean Medal would be presented at the forthcoming Anniversary Meeting to Prof. GEORG OSSIAN SÆRS, of Christiania, and the first presentation of the newly founded Trail Award for research on protoplasm, would be made on the same occasion to Prof. EDWARD ALFRED MINCHIN, Professor of Protozoology in the University of London.

The following were recommended by the Council to serve as Auditors for the Treasurer's Accounts, and by show of hands duly elected :—

For the Council: Prof. J. P. HILL and Mr. JOHN HOPKINSON.

For the Fellows: Mr. HERBERT DRUCE and Mr. JAMES GROVES.

The General Secretary exhibited a fruit recently bought by Mr. William P. D. Stebbing at a fruiterers in Jermyn Street, of unassigned origin, with the native name of "Cupu-assu." This name appears in the 'Flora brasiliensis' as applied to *Theobroma grandiflora*, Schum., a congener of the plant yielding chocolate, *T. Cacao*, Linn. Prof. J. W. H. Trail remarked that "Cupua" was the native Brazilian name for plants of that genus, and that "assu" meant large. Mr. T. A. Sprague exhibited two specimens from the Museum of the Royal Botanic Gardens, Kew, which



were strikingly diverse in form, but he yet believed them to be the same species.

The following papers were read :—

1. " Elm Seedlings showing Mendelian Results." By AUGUSTINE HENRY, M.A., F.L.S.
2. " Foraminifera and Ostracoda from Funafuti." By FREDERICK CHAPMAN, A.L.S.

April 21st, 1910.

Dr. D. H. SCOTT, M.A., F.R.S., President, in the Chair.

The Minutes of the General Meeting of the 7th April, 1910, were read and confirmed.

Miss Winifred Elsie Brenchley, B.Sc. (Lond.), was admitted a Fellow.

Mr. Henry Smith Holden, B.Sc., Mr. Charles William Mally, M.Sc. (Iowa), Mr. Sydney Gross Paine, and Mr. Percy Alfred Talbot, B.A. (Oxon.), F.R.A.S., were proposed as Fellows.

Miss Nellie Bancroft, Mr. Sidney Guest, and Mr. John Charles Wilson, were severally balloted for and elected Fellows.

The President having explained that Mr. JOHN HOPKINSON, who had been elected an Auditor at the last Meeting, could not take part in the Audit, Mr. LEONARD ALFRED BOODLE was proposed in his stead, and, by show of hands, unanimously elected.

Mr. JOHN HOPKINSON, F.L.S., exhibited eight coloured plates, in quarto, of British Nudibranchs, which will be shortly issued by the Ray Society, and explained that they were from drawings by Messrs. Alder and Hancock.

Prof. Dendy and the Rev. T. R. R. Stebbing commented on these illustrations.

The Rev. T. R. R. STEBBING drew attention to a " Witch-knot " or " Witch-broom " on a Spruce Fir, *Picea excelsa*, Link, from Walton-on-the-Hill, Surrey, where it had been detected by his nephew, Mr. William P. D. Stebbing.

A discussion followed in which Mr. A. D. Cotton, Mr. H. W. Monckton, Treasurer and Vice-President, Prof. F. W. Oliver, Mr. John Hopkinson, and the President took part.

The following papers were read :—

1. " The Anatomy of *Welwitschia mirabilis*, in the Seedling and Adult states." By Miss M. G. SYKES. (Communicated by Prof. H. H. W. PEARSON, M.A., Sc.D., F.L.S.)

2. "Die von Herrn Hugh Scott im Juli 1908-März 1909 auf den Seychellen gesammelten Anthomyidæ, mit den Gattungen *Rhinia* und *Idiella*." By Prof. P. STEIN. (Communicated by Prof. J. STANLEY GARDINER, M.A., F.R.S., F.L.S.)
3. "The Dermaptera of the Seychelles." By Dr. M. BURR, F.L.S., F.E.S.
4. "The Pteropoda and Heteropoda collected by the Percy Sladen Trust Expedition in the Indian Ocean." By Dr. J. J. TESCH. (Communicated by Prof. J. STANLEY GARDINER, M.A., F.R.S., F.L.S.)
5. "Die Pilzmücken Fauna der Seychellen." By Dr. G. ENDERLEIN. (Communicated by the same.)

May 5th, 1910.

Prof. E. B. POULTON, D.Sc., F.R.S., Vice-President, in the Chair.

The Minutes of the General Meeting of the 21st April, 1910, were read and confirmed.

Mr. John Charles Wilson, Mr. Thomas Parkin, Mr. Walter Ambrose Heath Harding, Mr. Robert Lawrence Heinig, and Mr. Iltyd Buller Pole Evans were admitted Fellows.

Mr. Cecil Hanbury was proposed as a Fellow.

Mr. E. M. HOLMES exhibited specimens of a rare British lichen, *Parmelia rugosa*, var. *concentrica*, Cromb., from the chalk hills between Eastbourne and Seaford, which had previously only been recorded from Melbury Hill near Shaftesbury in Dorset, where it was noticed in 1856 by Sir W. C. Trevelyan. This lichen grows in a concentric manner forming rounded nodules 1-2 inches in diameter, and is apparently formed at first on pebbles, but becoming detached and blown about by strong winds, ultimately forms more or less spherical growths.

He also exhibited specimens of the preserved fruits of a large variety of the Jujube, *Zizyphus Jujuba*, which is cultivated in China as a dessert sweetmeat, and is known by the name of "Mei-tsao," or honey-date. The fruits are preserved by boiling in honey, and are then pressed flat and dried and by mechanical means are given a striated appearance, having longitudinal lines from base to apex. The fruit has not as yet been imported into this country. It was received from Mr. McDougall of Swatow.

Mr. Holmes also directed attention to a volume of water-colour and pencil drawings, from which the plates of the very scarce work Postel and Ruprecht's 'Illustrationes Algarum' had evidently been prepared, the majority representing the plates being reversed, but also included some algæ which had not been utilised. The work consisted of only 200 copies, and the plates had been

destroyed by fire. Very few of these copies were held by private individuals, and the work, which was issued in 1840 at the price of £40, was hardly ever purchasable. The drawings shown were formerly in the possession of Mr. E. Meinshausen, of the Imperial Botanical Garden at St. Petersburg, and are now the property of the University of Birmingham.

Dr. OTTO STAFF, F.R.S., Sec.L.S., exhibited specimens of *Utricularia rigida*, Benj., from West Africa, and *U. neottioides*, St. Hil., from Brazil, the only known representatives of Kamienski's section *Avesicaria*, which is characterised by the absence of bladders. This condition seems to be correlated with the habitat of the plants, that is, rocks and stones submerged in running water. The plants are attached to the rocks or pebbles by modified clawlike rhizoids, very like the 'haptera' of Podostemonaceæ. The fertile stems are erect, bearing the flowers and fruits above the water. The assimilation-apparatus is submerged and consists of much-divided, in their ultimate divisions, capillary branches which resemble the 'leaves' of our native *Utricularias*. In *U. rigida* they seem always to spring from the base of the fertile stems and often attain a considerable length. Here and there they give rise to young fertile shoots which attach themselves by throwing out 'haptera' from their bases. In *U. neottioides*, however, they also spring from the axils of the lower 3-5 scale-leaves of the flowering stems, and remain rather short. Nowhere is any trace of bladders to be found. The flowers are those of typical *Utricularias*. The capsules are small and open, in *U. rigida* at least, by lateral slits, the valves remaining united at the top for some time. The seeds of both species are rather peculiar in the genus in as far as they exude mucilage when wetted. The coat of mucilage thus formed helps them in becoming fixed in positions suitable for the growing plant. *U. rigida* is known from the Sierra Leone coast to the head-waters of the Niger; *U. neottioides* from the mountains of Brazil (Bahia, Goyaz, Minaes Geraes). Thus they form another link connecting the floras of West Africa and Brazil. In habit these *Utricularias* resemble two other aquatic plants of tropical Africa also found in running water, namely *Quartinia*, a *Lythracea*, and *Angolea*, a *Podostemonacea*, of which specimens were shown.

An animated discussion followed in which the following took part:—Mr. E. M. Holmes, the Rev. T. R. R. Stebbing, Prof. Dendy, Prof. Poulton, and Mr. Henry Groves, Dr. Stapf replying.

Mr. F. N. WILLIAMS brought up for exhibition fresh specimens of a straw-coloured variety of *Lathræa Squamaria*, Linn., from Harefield, Middlesex, growing upon elm-roots; the normal form grew also with it, but was earlier in its development than the variety now shown, and besides had the property of quickly turning black after being gathered, whilst the new variety retained its hue for more than 24 hours without much change; it was distinct from the pure white variety *nivea*, known on the Continent.

The General Secretary exhibited the Linnean MS. 'Spolia botanica' dated 1729, to show that the name *Linnaea* had been scratched out, and *Rudbeckia* substituted, in compliment to Prof. Oluf Rudbeck the Younger, in whose house he was then living as tutor. This shows that Linnæus had early selected the plant which now bears his name, for he mentions two localities in Stenbrohult parish where it occurs, and that the choice of this plant to bear his name was not made when gathering specimens at Tugganforsen in Lyksele Lappmark.

After this conclusion had been arrived at, and the erasure and substituted name shown to several Fellows on the 10th March, 1910, the discovery was made that Dr. E. Ährling had recorded the same, which had been overlooked as being in a note in his 'Carl von Linnés Ungdomsskrifter,' i. pp. 92-93, of which the following is a translation:—"As regards the name or word *Rudbeckia* just employed, there is this peculiarity, that in the original manuscript the word was evidently written there after erasure, and of the first writing there remains a perfectly plain L such as Linnæus usually wrote, altered to R. Perhaps this suggestion may be ventured, that Linnæus first wrote *Linnaea*, when he meant to keep these records to himself, but afterwards, when he dedicated them to Prof. L. Roberg (into whose hands however the manuscript perhaps never came), he considered himself bound to protect himself against people's ridicule."

Mr. H. W. Monckton and the Rev. T. R. R. Stebbing raised questions, which were replied to by the exhibitor.

The following papers were read:—

1. Eight months' Entomological collecting in the Seychelles." By H. SCOTT, F.L.S.
2. "Some points in the Anatomy of the Larva of *Tipula maxima*; a contribution to our knowledge of the respiration and circulation in Insects." By J. M. BROWN, F.L.S.

May 24th, 1910.

*Anniversary Meeting.*

Dr. D. H. SCOTT, M.A., F.R.S., President, in the Chair.

The Minutes of the General Meeting of the 5th May, 1910, were read and confirmed.

Before opening the business of the Meeting, the President spoke of the incalculable loss which the Society, in common with the whole Empire, had suffered by the death of His late Majesty King Edward, Patron of the Society. The grief universally felt had found expression in every quarter; there was one remark however, which he, as President of the Society, would like to add.



His late Majesty's great work, in maintaining the peace of the world, claimed in a special manner the gratitude of scientific men, for Peace was the greatest scientific interest. Science, like genius, was of no country, and the maintenance of harmonious and friendly relations between all those nations among whom science was cultivated, was an essential condition for the advancement of knowledge.

The President then read from the Chair the following Loyal Addresses, which had been prepared by the Officers, and approved by the Council; these were unanimously adopted, all present rising from their seats :—

### To the King's Most Excellent Majesty.

THE HUMBLE ADDRESS OF THE PRESIDENT, COUNCIL,  
AND FELLOWS OF THE LINNEAN SOCIETY OF LONDON.

**Most Gracious Sovereign,**

WE, Your Majesty's most dutiful and loyal subjects, the President, Council, and Fellows of the Linnean Society of London in Anniversary Meeting assembled, humbly beg leave to offer our deepest and most heartfelt sympathy with Your Majesty in the great sorrow which has befallen You in the death of Your beloved Father, our late Sovereign Lord, King EDWARD VII. Your Majesty's loss is our loss also, and is felt not only throughout the Empire over which His late Majesty ruled, but by the world at large.

While thus expressing our sorrow, we ask leave, Sire, at the same time to tender to Your Majesty our unfeigned and heartfelt congratulations upon Your Majesty's accession to the Throne of Your Ancestors.

The sympathetic interest, which Your Majesty has constantly manifested in all that concerns the progress of Science, encourages us to hope that Your Majesty will be graciously pleased to continue to our Corporate Body, that beneficent Patronage which it has uninterruptedly enjoyed at the Hands of Your Majesty's Royal Predecessors since the granting of our Charter in 1802.

That Your Majesty's Reigu over a loyal, grateful, and loving people may be long and glorious, is our earnest wish and ardent prayer.

Given under the Common Seal of the Society, this twenty-fourth day of May, in the year one thousand nine hundred and ten.

DUKINFIELD H. SCOTT, *President.*

L. S.

ARTHUR DENDY,

B. DAYDON JACKSON, } *Secretaries \*.*

\* (Dr. STAFF being abroad could not sign the addresses.)

## To Her Most Excellent Majesty Queen Alexandra.

THE HUMBLE ADDRESS OF THE PRESIDENT, COUNCIL,  
AND FELLOWS OF THE LINNEAN SOCIETY OF LONDON.

Madam,

WE, the President, Council, and Fellows of the Linnean Society of London, in Anniversary Meeting assembled, remembering with heartfelt pride the high distinction which Your Majesty has conferred upon our Society in graciously consenting to become one of our Honorary Members, beg leave humbly to express our profound sorrow at the great and irreparable loss which has befallen Your Majesty, the Royal House, and the Nation, in the death of our Beloved and Venerated Sovereign Lord, King EDWARD the Seventh, our Patron, Whose Memory will ever be faithfully cherished by a grateful people.

Given under the Common Seal of the Society, this twenty-fourth day of May, in the year one thousand nine hundred and ten.

|       |                                        |                       |
|-------|----------------------------------------|-----------------------|
|       | DUKINFIELD H. SCOTT, <i>President.</i> |                       |
| L. S. | ARTHUR DENDY,                          | } <i>Secretaries.</i> |
|       | B. DAYDON JACKSON,                     |                       |

Mr. Frank Armitage Potts was admitted a Fellow.

Dr. Wilfred Eade Agar, M.A. (Cantab.), was proposed as a Fellow.

The Treasurer then brought forward the Annual Cash Statement to the 30th April last, duly audited, and explained the various items. The statement was thereupon received and adopted (see pp. 62, 63).

The General Secretary laid his Annual Report before the Meeting:—

Since the last Anniversary Meeting 18 Fellows had died, or their deaths been ascertained:—

William Hadden Beeby.  
Thomas William Bridge.  
Woodyer Merricks Buckton.  
Edward Clapton.  
Emmeline Crocker.  
William Henry Dallinger.  
William Hillhouse.  
Walter Bessemer Longsdon.  
Peter MacOwan.

Robert Morton Middleton.  
The Marquess of Ripon.  
Edward Saunders.  
George Sharpe Saunders.  
James Ebenezer Saunders.  
Richard Bowdler Sharpe.  
Arthur Ernest Bousfield Steains.  
Sir Charles Strickland, Bt.  
Edward Perceval Wright.

## TREASURER'S ACCOUNT FOR THE YEAR ENDING APRIL 30TH, 1910.

(Presented at the Anniversary Meeting, May 24th, 1910.)

*Receipts and Payments of the Linnean Society from May 1st, 1909, to April 30th, 1910.*

| <i>Receipts.</i>                        |           | <i>Payments.</i>                               |            |
|-----------------------------------------|-----------|------------------------------------------------|------------|
|                                         | £ s. d.   |                                                | £ s. d.    |
| Balance at Bankers on 1st May, 1909:—   |           | Taxes and Insurance .....                      | 10 18 9    |
| General Fund .....                      | £197 3 2  | Repairs and Furniture .....                    | 53 5 3     |
| Westwood Fund .....                     | 9 9 7     | Coals, Electric Current and Gas .....          | 45 10 6    |
|                                         |           | Salaries .....                                 | 769 11 0   |
| Donation from Professor Trail for Award |           | Library:—                                      |            |
| Interest on Investments:—               |           | Books .....                                    | £225 9 0   |
| General Fund .....                      | £228 10 8 | Binding .....                                  | 94 3 9     |
| Westwood Fund .....                     | 6 19 4    |                                                |            |
| Trail Award Fund .....                  | 1 15 0    | Expenses of Publications:—                     |            |
|                                         |           | Printing.....                                  | £802 10 3  |
| Admission Fees .....                    | 237 5 0   | Illustrations .....                            | 336 18 9   |
| Annual Contributions .....              | 204 0 0   | Distribution .....                             | 41 13 8    |
| Compositions .....                      | 1553 14 0 |                                                |            |
| Sales of Publications:—                 | 474 0 0   | Miscellaneous Printing and Stationery .....    | 1181 2 8   |
| Transactions .....                      | £113 9 4  | Petty Expenses (including Tea and Postage) and | 90 12 9    |
| Journals .....                          | 97 11 6   | Linnean Medal .....                            | 107 10 11  |
| Proceedings and Catalogues .....        | 4 4 3     | Investment of Compositions .....               | 189 8 8    |
|                                         |           | Trail Award Fund Investment .....              | 97 12 6    |
| Miscellaneous Receipts.....             | 215 5 1   | Do. Medal and die .....                        | 3 11 6     |
| Donations in aid of Publications .....  | 17 1 9    | Balance at Bankers, 30th April, 1910:—         |            |
|                                         | 64 0 0    | General Funds .....                            | £186 1 5   |
|                                         |           | Westwood Fund.....                             | 16 8 11    |
|                                         |           | Trail Award Fund .....                         | 16 6 0     |
|                                         |           |                                                | 218 16 4   |
|                                         |           |                                                | £3087 13 7 |





The deaths of four Foreign Members have also been recorded :—

Alexander Agassiz.  
Anton Dohrn.

Emil Christian Hansen.  
Kakichi Mitsukuri.

The following 13 Fellows have withdrawn :—

Arthur James Dicks.  
John Basil Feilding.  
Samuel Jennings.  
Samuel Lithgow.  
George Edward Lodge.  
Philip Walker Mackinnon.  
Frederick Gymer Parsons.

Albert Henry Pawson.  
Henry Power.  
Selmar Schönland.  
George Swainson.  
Henry Sullivan Thomas.  
Lt.-Col. John William Yerbury.

Mr. Alfred Woodward has been removed from the List by order of the Council.

31 Fellows (of whom 29 have qualified) and 1 Associate have been elected.

The Librarian's report was then read, showing that during the past year there have been received as Donations from Private Individuals 75 volumes and 189 pamphlets.

From the various Universities, Academies, and Scientific Societies, there have been received in exchange and otherwise 336 volumes and 86 detached parts, besides 68 volumes and 15 parts obtained by exchange and as donations from the Editors and Proprietors of independent periodicals.

The Council at the recommendation of the Library Committee have sanctioned the purchase of 184 volumes and 91 parts of important works.

The total additions to the Library are therefore 663 volumes and 381 separate parts.

The number of books bound during the year is as follows :—  
In full morocco 8 volumes, in half morocco 217 volumes, in half calf 3 volumes, in full cloth 338 volumes, in vellum 40 volumes, in buckram 15 volumes, in boards or half cloth 9 volumes. Relabelled (half morocco and cloth backs), 42 volumes. Total 672 volumes.

The General Secretary having read the Bye-Laws governing the elections, the President opened the business of the day, and the Fellows present proceeded to vote for the Council for the ensuing year.

The Ballot having been closed, the President appointed the Rev. T. R. R. Stebbing, F.R.S., Prof. M. C. Potter, and Mr. W. Fawcett, Scrutineers, who, having cast up the votes and reported to the President, he declared the Council to be elected as follows :—

E. A. NEWELL ARBER, M.A. ; HENRY BURY, M.A. ; Sir FRANK CRISP ; Prof. ARTHUR DENDY, D.Sc., F.R.S. ; Prof. J. B. FARMER, F.R.S. ; Dr. G. HERBERT FOWLER ; Prof. J. STANLEY GARDINER, F.R.S. ; ARTHUR W. HILL, M.A. ; Prof. J. P. HILL, M.A., D.Sc. ; JOHN HOPKINSON, F.G.S. ; Dr. B. DAYDON JACKSON ; HORACE W. MONCKTON, F.G.S. ; Prof. FRANCIS W. OLIVER, F.R.S. ; Prof. E. B. POULTON, F.R.S. ; Dr. A. B. RENDLE, F.R.S. ; Dr. WALTER GEORGE RIDWOOD ; Miss EDITH R. SAUNDERS ; Dr. DUKINFIELD H. SCOTT, F.R.S. ; Dr. OTTO STAFF, F.R.S. ; Miss ETHEL N. THOMAS, B.Sc.

The Ballot for the Officers having also been closed, the President appointed the same Scrutineers, who, having cast up the votes, reported to the President, who declared the result as follows :—

*President* : Dr. DUKINFIELD H. SCOTT, M.A., F.R.S.

*Treasurer* : HORACE W. MONCKTON, F.G.S.

*Secretaries* : Dr. B. DAYDON JACKSON,  
Prof. A. DENDY, D.Sc., F.R.S.,  
Dr. OTTO STAFF, F.R.S.

The President then referred in a few words to the losses the Society had sustained by death during the past year. He also announced to the Fellows the generous gift to the Society of £200 by Sir Frank Crisp, for the encouragement of Microscopical Research. The regulations adopted enjoin the award to be made by the Council at intervals of five years, for the best paper in our publications during the previous five years, contributed by our own Fellows, the first award to be made in May 1912, and its title to be the "Crisp Award for Microscopical Research."

The Constitution of the Crisp Award for Microscopical Research is as follows :—

1. The Award to be made at intervals of not less than five years.
2. The Award to be given by the Council for the best paper dealing with Microscopical Research.
3. The Award to be confined to Fellows and to work published by the Linnean Society since the previous award, and in the first case during the five years previous.
4. The first Award to be given in May 1912.
5. The Award to be paid out of the accrued interest on the £200, and to be accompanied by a bronze medal similar to the Trail Medal, but bearing the words "Crisp Award for Microscopical Research."
6. The fund to be invested in Metropolitan Water Board B Stock.
7. With regard to the procedure in the event of the Award being withheld, the provisions of the Trail Award to be followed.

The President then delivered his Annual Address as follows :—  
LINN. SOC. PROCEEDINGS.—SESSION 1909-1910. *f*

## PRESIDENTIAL ADDRESS, 1910.

I PROPOSE to take as the subject of my Address this year "Some Modern Ideas on the Course of Evolution of Plants"—an extensive field, no doubt, which it will only be possible to sketch in the merest outline on an occasion like this. The bearing of recent investigations in Fossil Botany on the problem will come in for a good deal of attention, but obviously it is impossible to limit oneself to this point of view.

I do not intend, however, to enter with any freedom upon the regions of pure theory, in which we must include the great question of the origin of the Alternation of Generations, characteristic of the higher plants.

Dr. Lang, it is true, in the remarkable paper which we discussed in February 1909, held out hopes of putting this question on an experimental basis; it will be extremely interesting to see what comes of this suggestion when practically tested, but I think that much will always remain hypothetical. As Dr. Lang himself recognized, we can hardly hope to reconstruct the conditions under which the sexual and asexual phases first became differentiated, a process which must have taken place ages before the date of our earliest fossil records.

I may venture, however, to state my conviction that the position of this question, so clearly put before us on the occasion referred to, is now far more hopeful and stimulating to research than it was a few years back. Dr. Lang said:—"If this ontogenetic view is correct, we should be justified in seeking for correspondence in the vegetative organs, and possibly also in the reproductive organs, between two individuals of the same life-cycle. These correspondences—though between haploid and diploid individuals—I should term homologies, since they may amount to practical identity when the conditions of development are exactly the same" \*.

It is not very long since the idea of any homology between the sexual and asexual generations would have been scouted by our more orthodox morphologists; even the heterodox would have hesitated to back their opinions so far as to seek for detailed correspondence. The old antithetic theory not only set up an impassable barrier between the two generations, it also shut off the vascular plants absolutely from everything below them. The sporophyte, *i. e.* the plant itself, was assumed to be a new intercalation in the life-history, and could therefore never be compared with the plant in Thallophytes, which was supposed to belong to the other generation. Now all this is changed—the Alga *Dictyota* has given the clue, for it shows us how the two alternating generations, the sexual and asexual, may be exactly

\* Discussion on "Alternation of Generations," 'New Phytologist,' vol. viii, 1909, p. 106.

alike, and homologous one with the other in all their parts, in cases where they are exposed to like conditions.

Thus the idea has once more gained ground that the vascular plant—the sporophyte—like the gametophyte or prothallus, may itself be a modified thallus: in this way the whole field of comparison between the higher plants and the Thallophyta is once more opened up. This conception adds enormously to the interest of the older types of vascular plants, for there is now always the possibility that among them we may succeed in tracing their organs—leaf, stem, and root—a step or two nearer to their origin. There was never the remotest chance of this so long as the plant was supposed to be derived from a sporogonium, for it was obvious that the farther back we went in geological history, the less like a sporogonium did plants prove to be.

As we shall presently see, the new views of alternation, involving the thalloid origin of the vascular plant, have already proved fertile in evolutionary ideas: the palæontologist, however, will do well to maintain a cautious position with regard to the application of these conceptions to fossil plants. Though we may now have a tenable theory of the origin of vascular plants, and it is theoretically possible that we may be able to trace some of the stages in their evolution from thalloid ancestors, it is yet extremely doubtful whether the fossil record goes far enough back to help us appreciably in such an attempt. It cannot be too strongly emphasized that the earliest known land-plants were already highly advanced and varied types, very far removed from any thalloid ancestry. It is possible that here and there a primitive character may have lingered, but the presumption is always against it.

I shall therefore only touch on theories of the derivation of vascular plants in so far as they affect our views of the inter-relations of their main groups, for it is on questions of the latter kind that new light has been thrown by the investigations, largely palæobotanical, of the last few years.

I may here refer to a remark of Dr. Gaskell's, in his opening speech in the discussion on the Origin of Vertebrates, to which we listened with such deep interest a month or two back. Dr. Gaskell used these words:—"It seems to me highly probable that this same law of upward progress, viz., that each successive group has arisen from some member of the highest group existing at the time, holds good also for the Vegetable Kingdom, especially in view of the statement recently made that Phanerogams arose from Cycads. I hope that the President may see his way to offer a few remarks on this aspect of the question"\*.

There was no time then to deal with Dr. Gaskell's point, and I promised to refer to it at the Anniversary Meeting. The question

\* Discussion on the Origin of Vertebrates, Proc. Linn. Soc., Session 122, 1909-10, p. 12.

raised is an interesting one, and it is worth while to consider how far Dr. Gaskell's suggestion is supported by palæobotanical and other data.

For one thing, to return to a subject already touched on, the present trend of opinion on the origin of the alternating generations is favourable to the hypothesis put before us by Dr. Gaskell. On the old antithetic view the plant (sporophyte) of the Vasculares was held to be derived from a sporogonium of the simplest type, the Liverwort *Riccia*, in which the asexual generation is merely a group of spores enclosed in an epidermis, affording the nearest analogy. Not only the higher Bryophyta, but all the more advanced Thallophyta were put on one side, their highly organized soma belonging, as it appeared, to the wrong generation; the leading races of plants, so far as their principal phase, the sporophyte, is concerned, were supposed to have started *de novo* from the elaboration of a zygote—a fertilized ovum. The sexual generation of the ancestral form was also assumed to have been at a low grade of organization, as shown in the prothallus. Now, as we have seen, the somewhat academic belief that "the plant is nothing but a sporogonium" is being abandoned, and the reasonable doctrine that the cormophyte is a more highly differentiated thallophyte is beginning to prevail. On this view the probability is that the Pteridophyta had their origin from the higher Thallophyta.

This, however, is of necessity all an hypothesis, far more probable than the former one, but still too much "in der Luft" to afford any very sure support to further hypotheses. Let us go on to the actual evidence.

What do we know about the origin of "successive groups" of plants? We are only concerned with the land-flora, for the evolution of marine plants is entirely a question for the future. We can go back no further than the Devonian. At that period we have good evidence that the following main groups of vascular plants were already in existence:—

Lycopods (Club-mosses).  
Equisetales (Horse-tails).  
Sphenophyllales.  
Ferns.  
Pteridosperms (Seed-ferns).  
Cordaitales.

Of these six great groups the Pteridosperms and the Cordaitales must be accounted the highest, for they were seed-bearing plants. The successive groups of later origin were, essentially, three in number, namely,

Cycadophyta,  
Conifers,  
Angiosperms.

The first two groups appeared, so far as we know, about the



same time, towards the end of the Palæozoic period—the Angiosperms much later, high up in the Mesozoic.

The origin of the Cycadophyta (an enormously numerous, varied and advanced class in Mesozoic times) is generally admitted—they came from the Pteridosperms, or Cycadofilices, to use the older name, to which Potonié and some others still adhere. Even Prof. Chodat \*, who is inclined to break up the Pteridosperms, would admit that some of the plants grouped under that name were on the line of descent of the Cycadophytes. Hence that great and dominant class of Mesozoic plants appears to have been derived from a highly organized preceding group, and in fact from one of the two highest classes of Palæozoic plants—a conclusion wholly favourable to Dr. Gaskell's view.

The question as to the Conifers is far less simple. There are three theories in the field :—

1. All Conifers may have come from Lycopods.
2. All may have come from Cordaitales.
3. Part may have come from Lycopods and part from Cordaitales.

I am not going to enter into the controversy now, but we will see how the different views affect the question before us.

If the Conifers as a whole were derived from the Palæozoic Lycopods, they came from a very highly developed earlier group, though not from the highest. The Lycopod advocates now base their case to a great extent on *Lepidocarpon*—a Lycopod which had attained to the seed-bearing habit, or something very like it. Further, the Lycopods, if not morphologically among the highest Palæozoic plants, were probably the dominant class of that age, at least in the coal-forests. So that on this view the Conifers had, at any rate, a very distinguished ancestry.

If, on the other hand, they sprang from the same stock with the Cordaitales, then they may perhaps claim as their ancestors the very highest of Palæozoic plants. It is not likely, however, that the typical Cordaites were themselves the direct progenitors of Conifers: they are too specialized—the fructifications of Cordaites, for example, were decidedly more advanced than those of Araucarian Conifers. The plants from which, on this hypothesis, the Conifers were derived were perhaps less highly modified than the true Cordaites, such as Grand'Eury and Renault investigated, though still very advanced types.

If, again, the Conifers had a mixed ancestry (not that I regard such a view as really tenable), they could claim kindred partly with the morphologically highest, partly with the most dominant race of the earlier period.

Thus, on any view, Dr. Gaskell's hypothesis finds support.

The Ginkgoales, a group of some importance in Mesozoic ages,

\* "Ptéropsides des Temps Paléozoïques," Archives des Sci. Phys. et Nat. t. xxvi. 1908, Geneva.

might be separately considered. They are almost certainly connected with the Cordaitales, and indeed more closely than the true Coniferae. The Maidenhair trees are not, however, of much consequence for our immediate purpose, for they cannot really be called a higher group than the Cordaitales, but are merely slight modifications of an old and persistent type.

We now come to the Angiosperms. Until quite recently no serious hypothesis as to their origin has been in the field, for comparisons with *Isoëtes* as regards some anatomical points, or with *Selaginella* as regards the endosperm, were obviously the merest analogies at the best. There was a vague idea in the air that their origin may have been obscure, from small, unimportant plants, easily overlooked or not preserved: so that they might have existed for a long time as inconspicuous members of the flora, side by side with the dominant Cycadophyta and Conifers. I think this was the current idea until Wieland, and his apostles Arber and Parkin, showed how we might well have had the ancestors of Angiosperms (or something like them) in our hands all the time without knowing it—that is, they showed that the Mesozoic Cycadophytes themselves, more than any other group, betray affinity with the great race which succeeded them. This important conception was suggested by the discovery that the fructifications of Bennettiteæ, the characteristic Mesozoic Cycadophytes, were organized essentially like the bisexual flowers of an Angiosperm, though, of course, with important differences in detail. The latest work has further strengthened the comparison, and there are strong grounds for the hypothesis that the Angiosperms arose from a stock nearly allied to such Mesozoic Cycadophyta as the Bennettiteæ. The view is by no means universally accepted: some botanists, as, for example, Miss Stopes and Prof. Fujii in their recent work on the Cretaceous Flora of Japan, still incline to the opinion that the Angiosperms may have sprung from unknown herbaceous plants with a simple floral structure. We cannot enter on the discussion here, but the Cycadophyte theory of the origin of Angiosperms is at any rate tenable, and, if confirmed, will afford a strong support to Dr. Gaskell's theory.

On the whole, though so much is still uncertain, one may safely say that the present tendency of botanical opinion, determined chiefly by palaeobotanical discovery, is favourable to the belief that new advances in organization start from the highest, or rather from very high, preceding types. Probably the latter, more guarded way of putting the case is the better; the *highest*, in the sense of the most differentiated types, may have been usually too far committed to special lines of adaptation to have afforded suitable material for new developments.

As a type of modern opinion on the evolution of the higher plants, influenced by the conception of the thalloid origin of the Cormophyte, we may suitably take the views of Prof. Lignier.

They were originally stated in his paper on the Sphenophyllales and Equisetales, published in 1903\*, and have been more fully developed in his recent essay on the Morphological Evolution of the Vegetable Kingdom (1908)†. Prof. Lignier is a strong advocate of the homologous theory of alternating generations. He derives all the higher plants (Bryophyta and Vasculares) from a hypothetical group, his Prohepatics, in which the life-cycle embraced two phases—sporophyte and gametophyte,—which, he suggests, probably also existed in their marine ancestors, as is the case in the recent *Dictyota*. He supposes that in the Prohepatics the sexual and asexual individuals had a flat dichotomous thallus, and that they were aquatic or semi-aquatic in habit. Typical antheridia and archegonia may have already appeared at this early stage; the author refers to their long persistence through so many of the higher groups—an example, as Prof. Buller has pointed out, of morphological stability dependent on adaptation. From the Prohepatic ancestors evolution proceeded in two main directions. In the Bryophyta the gametophytic phase assumed the greater importance, vegetatively, while the sporophyte became reduced in relation to its parasitic life. In the line of the Vasculares the reverse process went on: the gametophyte underwent progressive and ultimately extreme reduction, while the sporophytic phase showed an immense advance in vegetative organization, some of the branches of the thallus (cauloids) becoming specialized as organs of absorption, *i. e.* roots, which are acutely described as simply the most ancient of rhizomes. He cites the Lycopods as still showing traces of the common origin of aerial cauloids and roots.

Concurrently with this important differentiation of absorptive organs proceeded the no less momentous differentiation of an internal conducting system, by which these plants became vascular, and thus fully adapted to a terrestrial life.

As the sporophyte thus attained a more and more exuberant growth, a new differentiation of its aerial parts set in. The original dichotomous branching of the thallus became, as Prof. Lignier puts it, “sympodised,” certain branches becoming predominant, and thus forming an *axis*, while others were subordinated and constituted the beginnings of *leaves*. Here, however, a great distinction manifested itself, on which the main divisions of Prof. Lignier's system are based. In the Lycopodiaceæ the sympodisation of the thallus never went very far, and true leaves (limited thallus-branches) were never developed. The small leaves characteristic of the Lycopods—“phylloids,” as our author calls them—are not thallus-branches at all, but have always been of the nature of appendages and are derived from lamellar hairs already present at the Prohepatic stage. They are

\* “Les Equisétales et Sphénophyllales,” Bull. Soc. Linn. de Normandie, sér. 5, t. vii. p. 93. Caen, 1903.

† Comptes Rendus de l'Assoc. Franç. pour l'Avancement des Sci. 1908, p. 530.

comparable to the amphigastria of the Marchantias, and, indeed, to the leaves of the Bryophyta generally.

On the other hand, in the early members of the Fern series the sympodising process went on to a great extent, converting whole groups of thallus-branches (cauloids) into appendages, borne on the main supporting branches, and thus leading to the final differentiation of the thallus into stem and leaf. Small groups of terminal cauloids, forming part of the appendages, became flattened out into pinnules; a process which we can see exactly repeated in the modern Flora in plants which convert their branches into cladodes. Thus, according to our author, the Ferns and all the higher groups have true *leaves* differentiated out of thallus-branches, while the Lycopods alone retain the simple primitive appendages, which they possess in common with the Bryophytes. Hence all plants above the Thallophytes are divided into PHYLLOIDEÆ (Bryophytes and Lycopodineæ) and PHYLLINEÆ (Ferns and all remaining classes of vascular plants). In the former, the assimilating organs are still the lamellar phylloids of the supposed Prohepatic ancestors; in the latter they are true leaves, *i. e.* differentiated parts of the branch-system of the original thallus.

The Bryophytes are *gametophytic* Phylloideæ, the Lycopodineæ *sporophytic* Phylloideæ, the phylloid appendages being borne in the former on the sexual, in the latter on the asexual individual. Thus Mosses and Club-mosses find themselves at last united in one main group!

Prof. Lignier's idea is interesting. We see how a certain degree of affinity (though a remote one) may conceivably still be traced between members of the Bryophyte and Pteridophyte groups, which have been regarded as separated by the widest gap in the Vegetable Kingdom. But the point which more nearly concerns us is the separation of the Lycopods from the rest of the vascular plants. The possibility that the leaves of the Lycopods may be essentially different from those of the Ferns, is one which must have occurred to the minds of many botanists. I remember how, nearly thirty years ago, a walk through the Fern-houses at Kew suggested to me and to a botanical friend the idea that the fronds of Ferns might be really branches, and the leaves of Lycopods scales, comparable to the ramenta of Ferns. At that time, however, the relation of the Ferns to the higher classes of vascular plants was not recognized, so we never extended our idea to the leaves of Phanerogams.

Fossil Botany tends to emphasize the isolation of the Lycopods, for it shows us no transition between the microphyllly of this phylum and the megaphyly of other groups. On the contrary, it provides evidence, as Prof. Lignier has shown, that the *apparent* microphyllly of certain classes (Equisetales and Conifers for example) is derived from a primitively megaphyllous condition. There is no indication that this applies to the Lycopods; neither is there any appreciable evidence that their simple leaves ever became modified



into anything more complex \*. On the whole, Prof. Lignier's idea that the Lycopods stand apart from the rest of the Vasculares appears quite tenable, though by no means proved. It is confirmed by the simple relation between sporangium and sporophyll which prevails throughout the group, and by the fact that the Lycopods are the only vascular plants in which there is a want of sharp differentiation between root and shoot. The former character may not be a primitive one (Prof. Lignier himself regards the terminal position of the sporangium on a branch as the original arrangement; other botanists suggest the presence of a reduced sporangiophore); but the existence of so many transitional forms between root or rootlet and stem or leaf is a strong indication of a relatively primitive and isolated position.

I may here recall that Mr. Tansley has touched on the position of the Lycopods in a very illuminating way in the first and the last of his lectures on the Evolution of the Filicinean Vascular System †. He recognizes the peculiar character of their leaves, contrasting so sharply with the megaphylls of other Pteridophytes, but the explanation he suggests is different from Prof. Lignier's. He says that the Lycopods "may be independently derived from the primitive Propteridophytes by *foliar specialisation of short undivided branchlets of the thallus, instead of whole branch systems as in the Filicinean type*" (p. 9). This, as he points out, would bring the Lycopods into line with the other Pteridophytes without assuming any extensive reduction, or abandoning, in this case, the thallus-branch theory of the leaf, which he regards as by far the most rational and convincing which has yet been suggested.

I should like to dwell on the wonderfully instructive comparison which Mr. Tansley draws between the morphological construction of *Selaginella* and that of a Fern with its fronds, but must content myself with a couple of short quotations. "In *Selaginella* we have a very old if not a primitively microphyllous stock which modifies whole branch-systems for assimilating purposes. The leaf itself is so small as to exercise no influence on the general conformation of the vascular system, and corresponds physiologically with the ultimate pinnule or segment of the lamina in a fern-frond. But the branch-system as a whole retains its plasticity and becomes moulded on lines parallel with those of the fern-frond as a whole" (p. 135). "In the frond-like dorsiventral type of branch-system seen in some species of *Selaginella* we have in fact a kind of working model of the hypothetical thallus of the 'pro-Lycopod,' the leaves representing the ultimate assimilating branchlets, and the whole showing a convergence with a fern-frond hypothetically derived by integration of a whole thalloid branch-system" (p. 136).

To return to Prof. Lignier. The Phylloideæ are after all a limited group now, though so prominent in the Palæozoic Floras.

\* The doubling of the vascular bundle in *Sigillariopsis* is the only case in point, but does not seem to have led to anything further.

† 'New Phytologist,' Reprint, No. 2. Cambridge, 1908.



The great mass of vascular plants, which he derives from the Primofilices (Mr. Arber's name for the early Ferns), belong to the Phyllineæ, with true leaves, differentiated from thalloid branches or system of branches, as already explained. He divides the Phyllineæ into four groups:—

1. The Macrophyllineæ.—Leaves large and dominant in comparison with the stem. Primofilices and Ferns generally; Pteridosperms or Cycadofilices; Cycadophyta.
2. The Microphyllineæ.—Leaves reduced in comparison with the dominant stem. Cordaitæ; Ginkgoales; Coniferæ.
3. The Mesophyllineæ.—Leaves intermediate, as regards these relations, between groups 1 and 2. = Angiosperms. The latest developed of all the groups and the most highly adapted to special conditions, sometimes simulating Macrophyllineæ (*e. g.* Palms), sometimes Microphyllineæ (*e. g.* Heaths).
4. The Articulatæ.—Allied to the Macrophyllineæ, from which they became detached at the epoch of their ancestors, the Primofilices. Characterized by verticillate symmetry, progressive reduction of leaves, radiate arrangement of leaflets, and tendency to multiply the planes of cauline symmetry. This includes the Equisetales, mainly, and the Sphenophylls, wholly, a Palæozoic group.

It is at this point that Prof. Lignier's views have perhaps exercised the greatest influence on botanical opinion. A very few years ago it became customary to associate the Articulatæ with the Lycopods, for which fossil evidence seemed to speak, the characters in common being mainly anatomical. Prof. Jeffrey was the strongest advocate of this view, and, as is well known, divided all vascular plants into Lycopsidea and Pteropsida; the former including Lycopods, Equisetales and Sphenophylls, the latter all other Vasculares.

This classification was based partly on the microphyllly of the Lycopsidea, the megaphyllly of the Pteropsida, and partly on certain anatomical characters closely connected with the relative dimensions of leaf and stem. Other characters also came in, and the position appeared a strong one; at any rate I was among those who adopted it for a time. I now think, however, that the Equisetales and Sphenophyllales have been shown by Prof. Lignier not to be really microphyllous at all, but to be derived by reduction from plants with compound leaves of considerable size. The leaves of the Sphenophylls are generally of some complexity and often deeply divided—it is only their *segments* which have a simple character. *Archæocalamites*—the oldest known member of the Equisetales—had compound, forked leaves, while in *Pseudobornia*, a Devonian representative of the Articulatæ, the leaves were doubly compound, and were originally taken for fern-fronds. It seems clear from all this, and from the detailed arguments of Prof. Lignier, which I cannot now recapitulate, that the Articulatæ,

when microphyllous, are only so by reduction, and consequently that the anatomical characters correlated with microphylls are not essential to the group. On the other hand, as we have seen, the Lycopods stand apart as a genuine microphyllous class, unconnected by any known transitions with the large-leaved phyla. At the same time one cannot admit any very close relationship between the Articulatæ and the Ferns; their ancestors, though in all probability megaphyllous, may have been much less like Ferns than any of the known Primofilices. For these reasons, which I cannot now develop at greater length, it seems to me clear that the attempt to divide Vasculares into two main series only must be given up, at least for the present.

I have proposed a threefold division, into Pteropsida (Ferns and all Spermatophyta), Sphenopsida (Equisetales, Sphenophyllales, and Psilotales), and Lycopsidea (Lycopods alone). The isolation of the Lycopods while the Psilotales are put in Sphenopsida has been criticised, and justly so—the position of the little family Psilotales is a great difficulty, and I do not think we are yet in a position to solve it, in the absence of all geological evidence of their history. The group has certain definite characters in common with the Sphenophylls, namely the nature of the sporangial apparatus and the anatomy: for these reasons some modern authors have united them in one class. On the other hand, the Psilotales have other, less definite characters in common with the Lycopods—the dichotomous branching, the alternate leaves, and to some extent the habit (in the case of *Thesiopteris*). The older writers always put them in this class, but at that time the Sphenophylls were practically unknown. We are not at present able to reconcile the two apparent directions of affinity. It is best to emphasize the Sphenophyll relation as the more definite, and otherwise to reserve judgment.

It should be mentioned here that Prof. Lignier gives the Psilotales quite a different position, regarding them as the most primitive of the Lycopod series, and consequently of all living vascular plants. He believes that in the earliest Pteridophyta the sporangia were terminal on certain cauloids (derived from thallus-branches), and that the Psilotales only differ from this type in having the sporangia grouped on special short branches. In arriving at this opinion the author allows himself to be too much influenced by the very problematic Devonian fossil *Psilophyton*, of which we really know nothing definite. I cannot accept a view which ignores the points of agreement between the Psilotales and the Sphenophyllales, and the probability that the former have suffered some reduction in organization in consequence of their epiphytic habit.

Prof. Lignier is of opinion that all his Phyllocladæ (*i. e.* all Vasculares except Lycopods and Psilotales) are descended from the Primofilices.

As regards the Articulatæ, I have already suggested that if we accept this view we must take Primofilices in an extremely wide

sense. The comparison between the sporangiophores of *Sphenophyllum* and the fertile pinnules of the Devonian "Fern" *Archæopteris* \* does not seem to me very helpful, for *Archæopteris* can scarcely be regarded as one of the Primofilices, but appears to have been a very advanced type, possibly, as Mr. Kidston has suggested, a Pteridosperm rather than a true Fern.

With reference to the seed-bearing plants, however, I find myself in agreement with Prof. Lignier as to their ultimate origin from an early Filicinean stock. This is an opinion which has been very generally adopted, during the last few years, either for the whole or at least for a large part of the Seed-plants; formerly the Lycopods were in favour as the probable ancestral group, though the origin of the Cycads from Marattiaceous Ferns was taught by Sachs about the year 1880. It may be worth while to point out, in a few words, the grounds on which the modern view is held.

For nearly 30 years the existence of a considerable group of Palæozoic Fern-like plants allied to the Cycads has been recognized by some palæobotanists, though at first only on negative grounds, the plants in question showing no evidence of Filicinean fructification. Then came the discoveries of Williamson, Solms-Laubach and others, proving that many of these Fern-like plants had an anatomical structure intermediate between that of Ferns and Cycads. These observations strongly confirmed the opinion that the latter plants were derived from, or had a common origin with, the former; this stage in the development of our knowledge may be called the "Cycadofilices" phase. The discovery of the multiciliate spermatozoids of living Cycads, in 1896, further strengthened their affinities with Ferns.

Then in 1903 began the series of discoveries, led by Oliver and Kidston, proving that certain of the Cycadofilices bore seeds of a Cycadean type, and establishing a strong probability that this applied to the whole group, a group outnumbering the true Ferns of the period. The name *Pteridospermeæ* marks this important step in advance. That the Cycads sprang from a Fern-like ancestry was now established beyond reasonable doubt. The Cycads, however, were not merely the little isolated family that now bears the name—in Mesozoic times they were a vast and varied class of plants, for which Nathorst's wider name *Cycadophyta* is appropriate; for long ages they were a dominant race throughout the whole world. Thus the proof of the Fern-ancestry of this great class was already a serious matter. But the theory could not stop here. It had always been recognized, since the first recognition of the *Cordaiteæ* as a separate class, that these plants had a strong Cycadean affinity. It now further appeared that the seeds of the *Pteridosperms* were organized just on the same lines as those of the *Cordaiteæ*, while anatomically an almost unbroken series of transitions between the two groups has been traced. Hence it became evident that the *Cordaiteæ*—the most specialized of the

\* Lignier, "Sur l'Origine des Sphénophyllées," Bull. Soc. Bot. de France, sér. 4, t. viii. p. 278 (1908).

Palæozoic Spermatophyta—had a common origin with the Pteridosperms, and consequently that they also were ultimately derived from the Fern phylum. This meant that practically all the Palæozoic seed-plants (if we except the quasi-seminiferous Lycopods) were of Filicinean origin.

Up to this point I do not think there has been any very serious difference of opinion among modern botanists who have considered the question. I have already mentioned that the affinity of the Maidenhair trees with the Cordaitales is generally recognized. I am unwilling, at the close of this address, to embark on the controversy as to the origin of the Coniferæ, a question with which I have lately dealt in print. Strange to say, the morphology of the cone in this familiar order of plants is still very imperfectly understood; I am convinced that a thorough comparative re-investigation of the whole family will be needed before the question of their affinities can be cleared up. A vast amount of good work has been and is being done, but a broad synthesis is urgently needed.

There are so many points in common between the Conifers and the Cordaitales, both in the anatomy and in the morphology of the fructifications, especially the male, that I cannot doubt that these classes are allied, an opinion in which I believe nearly all botanists agree, though my friend Prof. Seward is a serious exception. If we accept this relationship, we cannot escape the conclusion that the Conifers were ultimately, though remotely, derived from the same ancient Fern-stock with the Pteridosperms and the Cycadophyta.

I have already touched on the great question of the origin of the Angiosperms. It is clear that if the views now so widely held of their relation to the Mesozoic Cycadophytes should be confirmed, they also must fall into line with the rest of the Spermatophyta. At any rate, without prejudging a problem which will long continue to engage the chief interest of botanical evolutionists, one may safely say that the only tenable or intelligible theory of the origin of Angiosperms at present before the scientific world, involves their derivation from the Cycad-Pteridosperm-Primofilices series, and demands for them a place among the Pteropsida.

My object in the very slight and rough sketch of a vast field, which I have ventured to lay before you, has been to bring home to the minds of the Fellows, especially such as are not themselves morphological botanists, the profound interest and importance of the fundamental problems of Descent which are now under investigation and even appear ripe for solution. It is the great merit of modern Palæobotany that it has put new life into the study of our phylogenetic questions. It has done so by forcing us into contact with realities, with the ancient plants themselves, which were the actual predecessors (though by no means always the ancestors) of our living Flora. I believe it to be true, in



spite of all those difficulties which spur us on to further research, that in Botany, as in Zoology, the doctrine of evolution rests at present most securely on a palaeontological foundation.

Mr. HENRY GROVES then moved:—"That the President be thanked for his excellent Address, and that he be requested to allow it to be printed and circulated amongst the Fellows," which being seconded by Prof. M. C. POTTER, was adopted by acclamation, and acknowledged by the President.

The President, then addressing Herr EMIL HVITFELDT, Secretary of the Norwegian Legation, said:—

Professor GEORG OSSIAN SARS is the distinguished son of a distinguished father, the late Professor Michael Sars having been one of the pioneers of deep-sea dredging; it was he who laid the foundations of our knowledge of the deep-sea Fauna.

Professor Georg Sars, following and extending the same lines of investigation, has long been recognized by his fellow-workers in all parts of the globe as a distinguished leader and guide. For nearly half a century his successive writings have been shedding light on the class of Crustacea in its different branches. Almost at the outset of his career he succeeded in rescuing a difficult group from the obscurity and confusion in which it had been previously involved. Experts have over and over again paid his systems of classification the supreme compliment of adopting them. His instructive essays on the larval Decapoda, founded on an ingenious but toilsome plan of investigation, would have sufficed alone to make a considerable reputation.

After showing his command of languages by treatises in Latin, French, and German, besides his native Norwegian, he has obliged us by adopting the English tongue for several important volumes. Above all, his ready pencil, in an almost miraculous number of scientific illustrations, has used an idiom which every nation can read with facility.

The carcinologists of Great Britain, I am informed, have special reason to rejoice that in Prof. Sars's crowning work on 'The Crustacea of Norway,' already containing 756 plates, by a fortunate coincidence, the fauna of their own country finds illuminating treatment.

All who have been privileged to be in communication with him, praise the courteous readiness with which he renders the assistance they desire, nor can anyone explore his writings without admiring the entire absence of unkindly criticism, and the generous acknowledgment of merit in the work of other students.

It is eleven years since we had the honour of enrolling Prof. Sars among our Foreign Members. It is now my agreeable duty, in recognition of the world-wide reputation which he has acquired, to present him, through your kind mediation, with the Linnean Medal, as a token of our highest esteem.



Herr HVITFELDT, in accepting the medal, stated that the Norwegian Minister himself would have been pleased to be present, but having only recently arrived in London, he had not yet been received in audience by His Majesty the King, and was consequently debarred from attending.

Turning to Prof. E. A. MINCHIN, the President said:—

Prof. EDWARD ALFRED MINCHIN,

It is a great pleasure to me to present to you, on behalf of the Society, the first Trail Award, generously founded by my friend Prof. Trail, "with the object of encouraging study that throws light on the substance known to us as Protoplasm, or on what may, in the progress of knowledge, be regarded in a corresponding way as the physical basis of life."

Your work, in various directions, has done and is doing much to throw new light on the morphology and developmental activities of the living substance of animal cells. You have made contributions of fundamental importance to our knowledge of the minute structure and development of the Calcareous Sponges. You have discovered the remarkable mode of development of those singular structures the calcareous sponge-spicules, and in addition to your valuable original memoirs on the group, you have written the very able account of these organisms in Sir Ray Lankester's great Text-book of Zoology.

You have also published much work of the utmost importance on the parasitic Protozoa, especially the Trypanosomes, a field of investigation of momentous practical significance as well as of the highest scientific interest. In furtherance of these studies you made an expedition to Uganda, to study the problem of Sleeping Sickness under the auspices of the Royal Society.

You have written a masterly treatise on the Sporozoa, for the Text-book already referred to, and in addition to all your own investigations, have rendered a further service to biological science by your translation of Bütschli's classical work on Protoplasm.

No one could more fittingly be the first recipient of the Trail Award, which I now present to you, for the recognition and encouragement of the study of the living substance of organisms.

Professor MINCHIN replied as follows:—

Mr. PRESIDENT, LADIES and GENTLEMEN,

I desire to express my deep sense of gratitude both to the Founder of the Trail Award for his generous benefaction, and to the Council of the Linnean Society for the honour they have done me in conferring the Award upon me. It adds very greatly to the pleasure and pride which I feel in receiving it, that the selection has been made by a body so distinguished and honourable as the Council of the Linnean Society.

The Trail Award is intended to encourage and promote the

study of protoplasm, this line of investigation being understood in its widest sense as the study of the living substance and its vital powers and manifestations. Strictly speaking, such investigations are co-extensive with the whole range of the biological sciences, but for convenience the study of protoplasm may be regarded as the special theme of that branch of scientific investigation which is occupied with cells and with organisms of simple structure, and which deals with their constitution, development, and elementary vital activities. In such objects we are confronted with the stupendous mystery of life under its thinnest veil, and we observe in bodies almost infinitely minute the exercise of the most extraordinary powers, such as would lead us to infer the existence of a very great complexity of organization. Thus a flagellum performs movements which necessitate the assumption of a complex structure, but after studying it with the best optical instruments and the most refined technique, we can only represent it by a bare pencil-line. The chromatin-substance of the nucleus exhibits marvellous activities and powers, but again our pencils can only draw meaningless dots. Nothing, again, is more wonderful than the fact that peculiarities in the complex mental and physical constitution of a human being should be transmitted from one generation to another through the nucleus of the spermatozoa, the tiniest cell in the body; but with all the technique at our disposal we can only represent that nucleus as a minute dense refringent body, apparently homogeneous. A consideration of such facts forces upon us the conviction that the living substance possesses a complexity of organization far transcending anything that our microscopes can reveal, and only to be inferred from the activities manifested by it.

Cells or unicellular organisms relatively higher in the scale possess various cell-organs for the exercise of different functions; but as we descend the scale in our survey of nature we see these organs stripped off, as it were, until we come to cells in which the living substance consists only of two parts, termed respectively the cytoplasm and the nucleus. This type of structure is far, however, from being the simplest possible condition of a living organism. The cell-nucleus itself is essentially a collection of grains of a peculiar substance known as chromatin, which is combined with various accessory structures, such as a framework, membrane, &c., and organized into a complex structural unit. In the simplest organisms there is no definite nucleus, in the strict sense of the word, but only scattered grains of the chromatin-substance. Hence the living substance, protoplasm, in its simplest form consists of two chief constituent parts:—

(1) Cytoplasm, a semi-fluid matrix, itself organized and exhibiting a minute structure which, according to the alveolar theory of Bütschli, is due to the arrangement of at least two distinct substances not miscible one with the other, forming the alveolar framework (reticulum) and the enchylema (cell-sap) respectively.

(2) Chromatin, occurring as minute granules imbedded in the cytoplasm, and either scattered in it, or aggregated wholly or in part to form a definite nucleus.

The question at once arises, which of these two constituents of protoplasm represents the true living matter? Is the cytoplasm, or the chromatin, to be regarded as the primary living substance?

No answer that may be attempted to this question can be regarded as in any way final in the present state of our knowledge, and the subject can hardly be discussed adequately in a brief space: but the following are a few of the most important facts upon which to found a judgment:—

I. No living organism is known with certainty which does not contain substance of the nature of chromatin: and some of the minutest organisms, *e. g.* some Bacteria and Spirochetes, seem to consist of chromatin alone. These facts indicate that chromatin is, and cytoplasm is not, a constant constituent of living bodies.

II. By experiment it is found that cells, if deprived of the nucleus and reduced to cytoplasm alone, cannot continue living for long, and cannot initiate vital changes or processes.

III. The present state of our knowledge tends to establish as a general truth that the chromatin-elements are the governing and directing bodies of cells, and as such are the bearers of hereditary tendencies.

From these data the conclusion seems to me irresistible, that chromatin is the primary living substance, not cytoplasm.

If then chromatin is a substance of such immense importance in living things, it becomes necessary to attempt to define or characterize it further—a very difficult task. In the first place, it is found that the chromatin-elements of the nucleus consist of, or contain, substances of greater complexity from the chemical point of view, than the other portions of the protoplasm. In the second place, this complexity is combined with a high degree of variability, as might indeed have been expected on general grounds. For since the vital activities and properties manifestly differ in every species of organism, and even, it might be said, in every individual cell, then, if the chromatin-substance be the regulating and determining cause of the vital activities and manifestations, it follows that the chromatin must differ to a corresponding degree in each case; and therefore no given sample of chromatin can be expected to be identically similar to any other sample. It is a matter of common knowledge that such differences do occur between the chromatin-elements of different organisms, and even in the same organism at different periods of the life-cycle; to take only the micro-chemical test most commonly employed for the identification of chromatin, namely its affinity for certain colouring-matters, it can be said at once that there is no stain which can be relied upon either to tinge the chromatin-elements of any organisms at all times, or to stain only chromatin.

In short, chromatin cannot be defined solely by chemico-physical tests: it is essentially a biological conception. By chromatin we understand certain grains of substance imbedded in the cytoplasm or aggregated in the nucleus, and playing a definite role in the life-cycle of the organism. In the first place, in reproduction of the simplest type by fission, the chromatin of the daughter-individuals is derived by growth and fission of the chromatin-elements of the parent individual. Secondly, in syngamy (sexual conjugation), the constant and essential feature of the process in all its innumerable variations is the union of chromatin from two distinct individuals. Thus chromatin exhibits in itself the primary vital properties of growth, reproduction and individuality—the individuality which is characteristic of living organisms, and which depends primarily on the variability of the living substance. A given granule in a cell cannot be determined with certainty to be chromatin by inspection or by chemico-physical tests, but only by its relation to the life-cycle of the organism. This is what is meant by saying that the conception of chromatin is a purely biological one.

Our notion of the living substance influences necessarily our ideas as to the primitive form of living organism. It has generally been held that the first living things were relatively large masses of protoplasm consisting of pure cytoplasm, without nuclear elements, which appeared later in evolution. Such hypothetical forms of life were termed *Monera* by Haeckel, and with a technique less advanced than that of modern times, this distinguished naturalist described organisms which he believed to be true *Monera*. But it seems practically certain that no organisms exist, however primitive, which do not contain in some form or another the chromatin-substance which is the essential constituent of a nucleus. On the view that chromatin represents the primary living substance, I believe that the first living things were excessively minute specks of matter, perhaps even ultra-microscopic. I consider that, of the forms of life existing at the present day, the earliest type is most nearly represented by the minutest *Bacteria* and allied organisms in which the body is practically nothing more than a grain of chromatin. The first stages of evolution consisted in a gradual increase in the size of the body, which came to be composed of several or numerous grains of chromatin imbedded in a matrix, the cytoplasm. With further growth in size, the chromatin, at first scattered through the cytoplasm (chromidial condition of the nuclear substance), became aggregated wholly or in part at one spot, and there became organized and combined into a compact body, the nucleus. With the separation of the nucleus and cytoplasm a most important stage of evolution was reached, namely the stage of the cell in the strict sense of the word, the starting-point of the evolution of the entire animal and vegetable kingdoms. It would, in my opinion, be of advantage, as conducing to clear thinking, if the term "cell" could be restricted in its application to that type of



organization in which there is a sharp differentiation of nucleus and cytoplasm, and if organisms such as Bacteria, in which there is scattered chromatin but no definite nucleus, were not termed cells. If, however, the term "cell" is too compromised to be restricted in this manner, then two terms should be coined to denote these two primitive grades or types of organization, the one without a nucleus, as in Bacteria and allied forms, the other possessing a nucleus, as in Protozoa and the cells of animals and plants.

I have ventured here to express definite views upon some very controversial and speculative subjects. It is not to be expected that everybody should be of one mind in such matters; but whatever may be the views taken, I think everyone will agree as to the fundamental importance of the study of the living substance in its simplest forms, and I desire, therefore, to express my strong appreciation of the sagacity and foresight, no less than the generosity, of the Founder of the Trail Award, and my thanks to him for the stimulus and encouragement which he has given to such investigations.

The General Secretary then laid Obituary Notices of deceased Fellows on the table, and the proceedings closed.

#### OBITUARY NOTICES.

ALEXANDER AGASSIZ.—The death of Alexander Agassiz leaves a real gap in the world of scientific oceanography which no man of our own generation can adequately fill. He died on March the 27th, 1910, on the steamer 'Adriatic,' *en route* from England to America, at the age of seventy-five. Alexander Agassiz was the son of Louis Agassiz, Professor of Zoology at Harvard. He accompanied his father to America in 1849, at the age of fourteen years, and graduated in Engineering and Zoology. His first serious work was on the Pacific Coast Survey of America, but in 1849 and in 1851 he had already served an apprenticeship under his father on the Atlantic sea-board and off Florida. He spent some years in assisting his father in developing the Museum of Zoology at Harvard; and he became well known for his ability not only in Zoology but in the management of affairs. In 1866 he undertook the development and management of the Calumet and Hecla Copper Mine on Lake Superior. This was at that time almost a worthless property: it has since become the greatest single copper mine in the world, and has paid in dividends, since that date, upwards of £25,000,000. His association with this mine led to the foundation of a fortune which enabled him to follow his natural bent towards Marine Biology. He retired from active work in connexion with the mine after about five years, but he retained his interest in it as President or Director until his death.

Professor Agassiz will be best remembered by his numerous



expeditions in the tropical regions of the world. His three cruises of the 'Blake' in the West Indies and through the Caribbean Sea produced two admirable volumes. They give an excellent idea of the topography of the Caribbean, with many notes and observations on the reefs of that region. In addition they give an account of the deep-sea fauna as well as chapters on deposits, the pelagic fauna, and the Gulf Stream. Even at the present day there is no better or more interesting book on the subject. Professor Agassiz paid further visits to the West Indies, particularly to the Bahamas, and the elevated coral-reefs of Cuba: he also explored the elevated reefs of Florida, and visited Hawaii and the west coast of North America, going down to the Galapagos Islands. In the latter cruise he was mainly interested in the surface population of the ocean, which he had previously investigated in the Gulf Stream. In 1896 Professor Agassiz visited, in the steamer 'Croydon,' which he had himself chartered, the Great Barrier Reef of Australia. He gave the world an admirable account of the structure of the reef. He confirmed Jukes's general analysis of its main features, but came to the conclusion that it could not have been formed by subsidence as was then generally supposed. In 1897 he visited the Fiji Islands, exploring most of the "live" and fossil reefs of that archipelago; in particular he examined the barrier reefs of the Lau Archipelago, putting down a boring on Wailangalala. He also examined many of the elevated coral limestone islands, thus getting many sections of reefs up to 1000 feet in vertical thickness. He calculated that the islands of the whole group had been elevated, had then remained nearly stationary, and that their present conditions might be explained by prolonged denudation and erosion. The "actual living reefs" were considered to be flats left by the erosion of a central island, while the lagoons had largely been formed by the scouring action of the sea. In 1899 Professor Agassiz was in the North Pacific, but in 1901 he visited the Maldive Archipelago in the SS. 'Amra,' visiting every atoll of that group. He gave us an important series of soundings between the different atolls of that archipelago, enabling us to get the first clear idea of its topography. In particular he inspected the northern atolls, which had not previously been examined. Professor Agassiz's next work was a long cruise which practically included every group of coral islands in the Pacific Ocean. This was followed by a dredging cruise down the eastern Pacific to Easter Island, during which all kinds of oceanographical research were carried out. The greatest interest is to be attached to these explorations, since the eastern Pacific is the largest area of the world absolutely uninterrupted by islands. An extensive, peculiarly barren area was discovered to the east of the Marquesas and Paumotu, and between these archipelagoes and the South American coast.

The Reports on these Expeditions were mostly published in the *Bulletin* and *Memoirs* of the Museum of Comparative Zoology

at Harvard. They are noted for their lucid statements of facts and for their excellent illustrations, which are made, to a large degree, to take the place of further description. They present a veritable mine of information to be drawn on by investigators interested in oceanic and other islands. The later expeditions were undertaken mainly to elucidate the problem of the formation of coral reefs. Professor Agassiz had at the time of his death practically completed his work on this important problem. He had hoped to present his book during the present summer: it had arrived at such a point that it is not likely to be long delayed. It should put the crown on forty years of continuous research, undertaken in every region of the world.

In addition to his oceanographical work, Professor Agassiz published many Monographs on Hydrozoa and Echinoderma. His first paper was on the "Embryology of the Starfish," in 1864, and he followed this up by reports on the Acalephæ and on the "Embryology of the Ctenophoræ." His "Revision of the Echini," 4to, 774 pp., with an atlas of 94 plates, 1872-4, is a classical publication for reference; while his last contribution to "Hawaiian and other Pacific Echini" was issued only last year. The variety and extent of his published works are very great, mostly in large and important Monographs giving the results of his collecting in the 'Blake' and in the 'Albatross,' U.S. Government steamers which he was allowed to run on paying their working expenses.

Alexander Agassiz was all his life connected in some way or other with the Museum of Comparative Zoology at Harvard. He succeeded his father as Director and Curator in 1874, and he built up the Museum from a small Institution to a magnificent home for his wonderfully rich collections from all parts of the world. He himself, to a large degree, built and endowed the Museum, which will ever remain as an enduring monument of his wonderful energy. Its publications he paid for on a most lavish scale and he equipped it with the most modern scientific apparatus. In his will he further bequeathed to it £40,000 with a possible residual share in his estate. His gifts to the Museum altogether probably reached at least £500,000. His full model of Funafuti atoll is a magnificent piece of work, while his vast collections will form, as it were, an immense library for future researchers in Oceanography.

In conclusion one must refer to the great charm of Professor Agassiz as a man. He belonged to no country but rather to the whole world of science; he was cosmopolitan in every sense, a Fellow or an Honorary Member of Scientific Societies in every country, a welcome guest everywhere; he was noted for his un-failing courtesy to all, to the most junior student as much as to the Professor or Director. To anyone researching on coral reefs he gave special encouragement. He was not prodigal of advice, but what he did give it would be wise to follow. He was not above taking advice from those junior to himself. He was a man of indomitable energy; subject to sea-sickness, many of his cruises

must have been great pain to him, but he never flinched if he might increase our knowledge of the science in which he was interested. He never sought recognition, and in many respects he was averse from it. He never liked teaching, and he seldom lectured anywhere. Like a true scientific man, he cared not for himself but simply for the advancement of the subject which he loved. He was elected a Foreign Member on the 6th May, 1875.

[J. STANLEY GARDINER.]

WILLIAM HADDEN BEEBY, F.L.S., F.R.M.S., was born on June 9, 1849, and died on January 4 of the present year. He was in the banking business, from which he retired only a few months before his death. From an early time he devoted his leisure hours to the study of British Botany, in which he acquired soon a reputation for acuteness and great critical knowledge. He added a considerable number of new forms to the 'London Catalogue,' and deposited some of his critical gatherings in the herbaria at the British Museum and at Kew. His publications in the shape of short articles and notes were mostly published in the volumes of the 'Journal of Botany' for 1879-1897 and for 1908. He was also engaged in the preparation of a Flora of Surrey. Lack of leisure, however, compelled him finally to entrust the work to other hands: but he wrote the Botany article for the Victoria History of the County of Surrey (1902). His Surrey collections as well as those made in Shetland, which he visited repeatedly, are to be placed in the Horniman Museum, in the botanical department founded by Mr. A. O. Hume. He was elected an Associate of the Society in 1887 and became a Fellow in 1890. A portrait of him was published in the 'Journal of Botany' for May 1910. [O. STAFF.]

EDWARD CLAPTON was born at Stamford, 28th September, 1830, died at his house "Tower Croft" on the 28th September, 1909, and was buried at Stamford on the 2nd October. He was the second son of his parents, and educated at the Stamford Grammar School, afterwards entering at St. Thomas's Hospital in 1850, after an apprenticeship to a local practitioner from the age of 16 to 20. In 1857 he became M.D. Lond. and F.R.C.S.; in the same year he was appointed Assistant Physician and Lecturer on Botany at St. Thomas's Hospital, and later on he lectured on *Materia Medica*. In 1858 he became Member, and 17 years later Fellow of the Royal College of Physicians.

Shortly before his death he presented to the Museum of the Royal College of Surgeons two branches and a bundle of twigs from the plane tree in the island of Cos, under the shade of which Hippocrates lectured on Medicine from a marble seat, still in existence: the tree is believed to be considerably more than 2000 years old.

He was elected a Fellow, 21st November, 1861. [B. D. J.]

EMMELINE CROCKER was born in 1858 at Dulwich, where her father Augustus Coleman Crocker then lived; when quite a child the family moved to Cheshunt, and here she grew up, with the exception of her school time at Brighton. On leaving school she continued her studies in music and in art, the latter she pursued with much ardour under Miss Gann at the Queen's Square School of Art.

Upon her mother's death, when the ties of home life became slighter, Miss Crocker undertook a trip round the world by herself, visiting on her way the botanical gardens of Singapore and Hong Kong, finally reaching home by way of Canada.

After her return she spent some time at Glasnevin, where she became conversant with practical garden work under Mr. F. W. Moore, A.L.S.; on leaving she became directress of Mr. Spottiswood's garden at Porthquidden in Cornwall, from time to time contributing articles to 'The World' newspaper, which were reprinted in 1908 as "Thirty-nine articles on Gardening." This estate was sold by the owner in 1907, and then Miss Crocker resolved to busy herself with a monograph on *Rhododendron* forms in cultivation, and for this she painted a large series of pictures.

Unfortunately she had suffered of late years from repeated attacks of influenza, and with the idea of escaping the English winter, she decided to spend that part of the year in Madeira. Here she devoted herself to the flora of the islands, and began a collection of marine Algæ for Kew, but owing to the bad sanitation of the hotel at Funchal where she was staying, a violent epidemic of typhoid fever broke out, and our late Fellow fell a victim to it, dying on the 26th February, 1910.

She was elected Fellow so recently as 6th February, 1907, but entered into the life of the Society with characteristic energy, attending our meetings and using the Library freely.

For the materials for the foregoing sketch the writer has to thank Mr. Alfred Crocker and Miss Alice Shaw. [B. D. J.]

The Rev. WILLIAM HENRY DALLINGER, LL.D., D.C.L., D.Sc., F.R.S., F.L.S., F.Z.S., F.R.M.S., &c., died on the 7th November, 1909; he was born at Devonport on July 5, 1840; he was the son of J. S. Dallinger, and married Emma J. Goldsmith, daughter of David Goldsmith of Bury St. Edmunds. At one time he thought of adopting Medicine as his profession, but his strong religious tendency led him, in 1861, to enter the Wesleyan Ministry; the first circuit to which he was appointed was that of Faversham, but he afterwards travelled those of Cardiff, Bristol, and Liverpool; his life, at this time, was that of a circuit Minister, but he occupied most of his leisure in studying Hebrew, Greek, and German.

In 1880 Dr. Dallinger was appointed Governor and Principal of Wesley College, Sheffield, in which capacity he was highly appreciated, but he resigned the post in 1888, and became a



minister without pastoral charge, in order to have more time at his disposal for his scientific studies and researches; these, and his lectures on microscopical and biological subjects, occupied the greater part of the last twenty one years of his life, but of late years he was greatly hampered by failing health. One of these lectures which was delivered in 1884, before the British Association at Montreal, was famous at the time: it was on "The lowest and smallest forms of animal life."

Dr. Dallinger was elected F.R.S. in 1880, and received the LL.D. from the Victoria University in 1884, the D.Sc. from Dublin in 1892, and the D.C.L. from Durham in 1896; he was President of the Royal Microscopical Society of London from 1884 to 1887 inclusive, and of the Quekett Microscopical Club from 1890 to 1892 inclusive; he was also senior lecturer on the staff of the Gilchrist Educational Trust. He was a Fellow of the Linnean Society from 2nd March, 1882, until the time of his death, and served on the Council from 1888 to 1890.

From early youth Dallinger took a strong interest in natural science, but the researches which established his position in the scientific world, and eventually made his name famous, commenced in 1870, and lasted for about ten years; they were microscopical researches on the life-histories of certain minute septic organisms known as "Monads." The results of these researches were published, from time to time, chiefly in the 'Monthly Microscopical Journal,' which was at the time the journal of the Royal Microscopical Society. The earlier of these researches were conducted in conjunction with J. Drysdale, M.D., and were marvels of patient and skilful investigation: the life-histories were traced and established by continuous watching through the microscope day and night without a break; one observer sitting down to the instrument as the other rose, until the whole life-history had been thoroughly traced and verified. The question of spontaneous generation was then a burning one, and the results of Messrs. Dallinger and Drysdale's enquiries and experiments had considerable influence in determining the conclusions at which the greater part of the scientific world arrived. Some of the later researches into the thermal death-point of known Monads and Monad-germs were conducted by Dr. Dallinger alone, without Dr. Drysdale's assistance; his careful and prolonged experiments proved that these Flagellates, which normally lived at a temperature of about 60° F., could, by a gradual raising of the temperature of the fluid in which they were immersed, be accustomed to live and thrive at 158° F. The joint experiments had already proved that the germs were capable of resisting a fluid heat of 220° F., and a dry heat of 300° F.

The enquiries above referred to were probably Dr. Dallinger's only original investigations and discoveries of importance in biological science; but in the course of them he had to employ the highest powers of the microscope, and to use them to the best advantage, as the flagella of living monads are difficult objects to



see and define properly; and, in his anxiety to give his investigations every advantage that patience and skill could afford, he studied the optical construction of the instrument, and the most advantageous methods of its illumination and management, until he became extremely skilful in its use and a great authority upon these subjects, in which he took a deep interest. It was probably this which led him to edit the 7th and 8th editions, published respectively in 1891 and 1901, of Dr. W. B. Carpenter's "The Microscope and its revelations." For the 1891 edition Dallinger entirely re-wrote the whole of the first seven chapters, being the part treating of the instrument itself and the preparation of objects for examination by its means, and the same portion was almost entirely rewritten for the 1901 edition. It is characteristic of the patient and untiring nature of the man that, during the transit of part of these manuscripts to the printer, an accident happened to the box and a considerable portion of the manuscript was lost: Dallinger at once quietly set to work to restore it.

Dallinger wrote his well-known "Fernel Lecture" on "The Creator and what we may know about Creation" in 1896; he frequently contributed scientific articles to the 'Wesleyan Methodist Magazine,' and he wrote some other papers of less importance from time to time.

When Dallinger was President of the Royal Microscopical Society he was also Principal of the Wesley College, yet he rarely missed a meeting of the Society, but used to travel back to Sheffield by the night-mail after the meeting in order to be ready for his duties at the College the next morning; and after his term as President expired he undertook the office of honorary optical Secretary in order to assist the Society, and this office he held for many years.

Finally, Dr. Dallinger was a man who gained the affection of most of those who knew him, and all those who were in any way associated with him in his scientific pursuits will remember his constant readiness to help others and his anxiety to acknowledge all assistance which he himself received. [ALBERT D. MICHAEL.]

FELIX ANTON DOHRN, Foreign Member of the Linnean Society since 1888, the founder of the famous Biological Station at Naples, died in Munich on September 26th last, in his seventieth year. He lived to see not only his own Foundation grow famous, the acknowledged rendezvous of biologists of all nations, but also similar institutions for the prosecution of marine research spring up on the shores of almost every civilized country with a sea-board. It is not too much to say that all these institutions, which are now to be counted by the score, owe their existence largely to the insight and courageous initiative of Anton Dohrn, who was the first to conceive the plan of a Marine Biological Station, and to prove it feasible in the face of much opposition and even ridicule. It is therefore difficult to overestimate the part which he has played in the great progress of marine biology during the past

forty years, a progress which has contributed as much as anything else to the general advance of biological science.

Although his principal life-work was the founding and management of the Station, he made many important contributions to Morphological Science, especially upon the vexed and complicated problem of the evolution of the Vertebrate head.

His quick perception of the trend of scientific thought had recently convinced him that biology in the future would turn more in the direction of experimental and physiological enquiry, and accordingly he determined to increase the buildings and staff of the Aquarium for the especial purpose of offering facilities for such work, and his friends may rejoice that he lived to see this undertaking, which involved the erection of a wing equal in size to the original building, most happily consummated.

It was Dohrn's wish to preserve the international character of the Biological Station. Great Britain has for many years been represented by students appointed by Oxford, Cambridge, and the British Association. In many conversations with Professor Dohrn I learnt that he regarded this connection with especial pleasure, owing to his intimate friendship with Huxley and Francis Balfour in the early days of the Station, and to the cordial support which they had given him in difficult times.

Built physically on a grand scale with immense reverberating voice, everyone who knew him felt that his mind corresponded: his bursts of humour, his explosions of anger, his ardent enthusiasms, were all irresistible in their spontaneous force. A man of great culture in literature and the arts, especially music, he never forsook right up to the end the slow and laborious method of science. In the power and destiny of science he possessed an ardent faith which amounted to idealism, almost to romanticism. His sense of the mysteries of nature and what they meant for man transcended the narrow bounds of knowledge, and any advance into the unknown, however small or apparently insignificant, was to him worth any amount of effort and sacrifice. This burning enthusiasm for knowledge was certainly the source of his greatness. Doubtless his wide sympathies, his knowledge of men and of the world, both great and small, his extraordinary faculty of linking powerful and distinguished men to his own enthusiasm, contributed largely to his success; but it would be the grossest error to ascribe the outcome of his life's work to a successful obsequiousness to those in power. The strength of his influence resided ultimately in the strength of his belief in nature and in science, without which his tact and knowledge of affairs would have accomplished little. By his death natural science has lost one of its most forcible and genuine leaders. [GEOFFREY SMITH.]

EMIL CHRISTIAN HANSEN was born at Ribe in Jutland, Denmark, on May 8, 1842, and died at Copenhagen on August 27 of last year. He was originally a house-decorator and pupil of the Art school at Copenhagen, but he soon turned to the study of science.

As he had, however, to earn his living as a private tutor, it was not until 1866—when the Danish government granted him a scholarship—that he could apply himself wholly to his studies. Having been appointed a science master in a Copenhagen gymnasium, he began to occupy himself more exclusively with botany and chemistry.

Apart from a preliminary communication on a peat moor in Denmark, a subject which he did not follow up, his first publication was on “*De Danske Gjødningssvampe (Fungi fimicoli Danici)*” in *Vidensk. Medd. Copenhagen*, 1876, pp. 207–354.

In 1878 he entered the physiological laboratory at Carlsberg, near Copenhagen, where he at once began that brilliant series of researches on fermentations which constitute his life-work. He initiated it with a dissertation, “Contributions to our knowledge of the organisms which are found in beer and beer wort and are able to live therein”; and having taken his doctor degree in 1879 he was appointed Director of the Carlsberg Laboratory, which post he held until his death.

Hansen's biological researches on the organisms of fermentation, and among them mainly of the *Saccharomycetæ*, were carefully planned and carried out on ingenious methods which assured a degree of precision not attained before. They were fruitful in theoretical results bearing on the biology of those micro-organisms, and in many respects of the physiology of the cell generally; but thanks to his practical genius, they also led in the industries depending on fermentation to technical improvements of the greatest importance and in some respect to changes which almost revolutionized them. His numerous publications are scattered through the *Comptes Rendus* of the Carlsberg Laboratory, the *Centralblatt für Bacteriologie und Parasitenkunde*, the *Annals of Botany*, the *Zeitschrift für das gesammte Brauwesen*, etc. An independent publication, “*Untersuchungen aus der Praxis der Gärungsindustrie*” (Practical Studies in Fermentation, Engl. transl. by Miller), did not get beyond part ii.

In 1898, E. C. Hansen was elected a Foreign Member of the Society. [O. STAFF.]

WILLIAM HILLHOUSE, whose death occurred at Malvern Wells on January 27th, 1910, was appointed to the professorship of Botany at the Mason Science College, Birmingham, in April 1882. He had for some time suffered from chronic pulmonary trouble, and in September 1909 he resigned his professorship at the University of Birmingham. Unfortunately he lived but a short time to enjoy his retirement.

He was born at Bedford on December 17th, 1850, and in course of time became an assistant at the Bedford Modern School. It was during this period that he began his study of Botany, working more especially the Bedfordshire flora, and he was instrumental in founding the Bedfordshire Natural History Society. He became a Fellow of the Linnean Society in 1876. In 1877 he

went to Trinity College, Cambridge, of which he soon became a distinguished scholar. From 1878 to 1882 he was assistant curator of the University Herbarium, and was in this period appointed a University Lecturer in Botany. He also became Lecturer in Botany to both Girton and Newnham Colleges, and his literary tastes combined with his general activity were largely responsible for the appearance of the 'Cambridge Review,' of which he was one of the original editors.

On his appointment at Birmingham, Hillhouse went over to Bonn to work with Prof. Strasburger, a visit which culminated in the translation of Strasburger's 'Practical Botany.'

In Birmingham and the Midlands Hillhouse took an active part in educational work. He was for a time president of the Birmingham Natural History Society, and of several of the local Institutes, and for years he was a prominent member of the Leicestershire Education Committee. He was honorary secretary, and subsequently chairman, of the Birmingham Botanical and Horticultural Society, and under his direction the Botanical Gardens, Edgbaston, became one of the delights of the Birmingham district. Until his death, which will be sincerely regretted by many students, friends, and colleagues, he was chairman of the Council of the Midland Reafforestation Association.

[G. S. WEST.]

Professor PETER MACOWAN, who died at Uitenhage, Cape Colony, on the 1st December, 1909, was born at Hull, Yorks., 14th November, 1830, and at the age of 16 became a tutor at Bath, and after one or two intermediate positions, became a master at Huddersfield College, acting also as teacher of Chemistry, in 1857, in which year he graduated in Arts in the University of London.

He had already taken up the study of Phanerogams and Mosses, when his health gave way, and threatened with lung trouble, he left Huddersfield in 1861 to take charge of a projected college at Grahamstown. His health was wholly re-established on the voyage out, and he never felt any serious failure until late in life. At this school, Shaw College, he began his botanical work in South Africa, and got into correspondence with prominent botanists at the Cape and elsewhere.

In 1869 he left Grahamstown to become science tutor at Gill College, Somerset East. He gave his herbarium to the College, and began a museum, and from this time must be dated his association with Dr. Harry Bolus, who was then living at Graaff Reinet.

He was appointed Director of the Capetown Botanical Gardens in 1881, and soon afterwards became Professor of Botany at the South African College. Here he seemed to have attained his true position, but the real position of the Botanic Garden was really most unsatisfactory, and the appliances at the College were quite inadequate, and after a few years his classes were perforce abandoned, though his methods were admirable and his descriptions

vivid. The Garden was carried on with much trouble, upon a precarious retail business in plants and seeds, and it is much to his credit that when in 1892 he handed it over to the municipality it was with a small credit balance, after defraying costly improvements.

The Herbarium in connection with the Garden was badly housed and had been greatly neglected. It was not till 1892 that a couple of rooms were assigned to it; in that year he reported that he had now 31 cabinets, against 7, which was the number in 1881, with 3000 sheets, and containing the types of the Cape Flora as far as Campanulaceæ, mounted by Dr. Harvey in 1864. The basis of the collection was once the property of Zeyher, and afterwards of Dr. Pappe, and dated from 1825 to 1849; the specimens, besides being old, had suffered much from want of care, and even Harvey's set had been allowed to lie for years unpoisoned, and it is due to Sir Henry Barkly that this measure of preservation was adopted. By 1901 there were 61 full cabinets, the increase entirely due to MacOwan's own hard work. In 1904, to his great delight, his old herbarium was presented to the Albany Museum by the Trustees of Gill College, where it had lain entirely unused since his time. Although it had somewhat dwindled, it still contained 14,000 sheets of phanerogams, many of them types, and 1800 fungi.

In 1884 he began with Dr. Bolus the issue of his 'Herbarium normale Austro-Africanum,' which was continued till 15 centuries had been issued; five more were issued by MacOwan single-handed.

He resigned the Directorship of the Botanic Garden at the end of 1891, but retained the post of Curator of the Herbarium till his retirement in 1905; when he left the sheets had increased to 44,000, of which 25,500 were Cape plants. For about two years subsequent he worked about 6 to 7 hours daily in the Herbarium of the Albany Museum, chiefly getting the Gill College herbarium into creditable order once more. In 1907 he had a slight paralytic stroke, and finding the climate of Grahamstown too cold in winter, he moved to Uitenhage. He gradually weakened till the end came last year, as previously noted, and passed away in the house of a son-in-law, Mr. Chase of Uitenhage.

He took his degree of B.A. before leaving England, and he never revisited his native land. Frequently acting as examiner to the Cape of Good Hope University in Chemistry, Botany, Geology, and Zoology, the Council bestowed upon him in 1902 the honorary degree of Doctor of Science. He was elected Fellow of our Society on the 2nd April, 1885.

For the particulars of the foregoing sketch the writer would acknowledge his indebtedness to Dr. Schönland's obituary of his father-in-law which came out in the 'Kew Bulletin,' 1910, pp. 84-90, and to the sketch with portrait which appeared in the 'South African Journal of Science' for January last, pp. 71-79, above the initials of Dr. Juritz, the Editor; the latter article contains a partial bibliography of Prof. MacOwan's writings. [B. D. J.]



ROBERT MORTON MIDDLETON was born at Sowerby, near Thirsk, Yorkshire, on January 25, 1846, and died on August 8, 1909, at Wallington, Surrey. His career, beginning in the banking business, was somewhat varied. He spent part of his life in the United States, whence he returned to England in 1896. Afterwards he went out on missionary work among the Araucarian Indians of South Chili, coming back to this country for good in 1907. Since then he was employed temporarily at the Botanical Department of the Natural History Museum, where he also placed his collection of Chilian plants. He was a lively, genial man of many interests; but he published very little. He joined the Society, whose meetings he attended very regularly, on the 4th March, 1880. [O. S.]

The Most Honourable GEORGE FREDERICK SAMUEL ROBINSON, 1st Marquis of Ripon, K.G., P.C., was born in London 24th October, 1827, son of the 1st Earl of Ripon and Sarah, only daughter of the 4th Earl of Buckinghamshire. He was elected a Fellow on the 20th November, 1849, as Viscount Goderich, and, continuing to pay his annual contribution during his life, was the oldest paying Fellow on the Roll, and had therefore contributed most largely to the pecuniary support of the Society.

Of his varied and honourable official career this is not the place to discuss: the places he filled in the State, including the Governor-Generalship of India from 1880 to 1884, claimed the whole of his time and attention. In addition to the Garter Knighthood and membership of the Privy Council, he was G.C.S.I., Hon. D.C.L. (Oxford), Hon. Litt. D. (Victoria), and F.R.S. He succeeded to the title in 1859; and died at his beautiful seat, Studley Royal, on the 9th July, 1909, the cause of death being heart-failure.

[B. D. J.]

EDWARD SAUNDERS, F.R.S., F.L.S., F.E.S., and GEORGE SHARP SAUNDERS, F.L.S., F.E.S., Hon.F.R.H.S., were both born at East Hill, Wandsworth, sons of the well known William Wilson Saunders, F.R.S., who was for eleven years Treasurer of the Linnean Society. They were further fortunate in having for their elder half-brother William Frederick Saunders (F.L.S. 1857-1901), not only an accomplished botanist, but a noble-minded man, who in all relations of life "loved himself last." In 1857 the family residence was removed to Hillfield, Reigate, and there the brothers, educated at home, were constantly in familiar touch with their father's immense entomological, horticultural, and other collections. Their father's ready sympathy with every form of scientific pursuit will be remembered by all who knew him; and the frequent presence at Hillfield of men illustrious in various departments of natural history was well calculated to impress the minds of intelligent boys with the importance of such knowledge. Between the claims of heredity and environment, it must remain an open question which was the more potent influence to produce in the

two brothers the tastes and aptitudes for which their father was distinguished. However that question may be answered, it is certainly a little, or not a little, remarkable that George, born March 9, 1842, and Edward, born March 22, 1848, published in joint authorship, through the Holmesdale Natural History Club, a "List of the Land and Freshwater Mollusca of the Reigate District," in February 1861. There is reason to believe that in this publication Edward Saunders, not yet thirteen years of age, was the predominant partner. A second edition, brought up to date, was issued in January 1864. The correspondence for exchange of specimens in which the younger of the two naturalists had at once become involved, though no doubt a trial for youthful vanity, was only a foretaste of the incessant appeals for friendly scientific aid which throughout his life he never failed to answer with unselfish readiness. At sixteen, by his "Coleoptera at Lowestoft" he opened on a new subject, which was thenceforward for several years to engage his special attention. This early contribution appeared in the first volume of 'The Entomologist's Monthly Magazine,' a useful serial destined to have him for the last thirty years of his life as one of its editors. In its March number for the present year there appears an admirably sympathetic appreciation of his work and character by his long-time friend and well-wisher, the Rev. F. D. Morice, M.A., formerly a master at Rugby. It is unnecessary, therefore, to repeat the details there given of his assiduous labours and numerous publications, successively on the Buprestidæ, the Hemiptera Heteroptera, and lastly on the Aculeate Hymenoptera. It may, however, be noticed as a token of the ardour with which he carried out his investigations, that when publishing in 1871 his important 'Catalogus Buprestidarum,' he had won the right to say in his Preface:—"To render the synonymy as full and accurate as possible, I have myself examined the types in the following collections:—British Museum, Museums of Berlin, Copenhagen, Kiel, Leyden, Oxford, d'Hist. Naturelle de Paris, Stockholm, and Upsala; Colls. Chevrolat, Kirsch, Le Conte, Linnæus, Mniszech, Reiche, Sallé, Thomson, Weyers."

Concerning his later efforts Mr. Morice writes:—"It is quite impossible within the limits of this Notice to give even the titles of Saunders's minor writings on Aculeates. It must suffice to say that his grand work 'The Hymenoptera Aculeata of the British Isles' (1896) is one of the few without which no serious Hymenopterist thinks his working-library complete, and that its merits have been acknowledged in the warmest terms by every one at home or abroad who is competent to form an opinion upon it." Among his minor writings on the subject, however, one of the latest is worthy of record, because it shows that he could at will descend from that impassioned sublimity of style, with which, as is well known, specialists are wont to soar over the heads of the vulgar. In 1908 he published with Routledge & Sons a pleasant little Tractate for the unlearned, illustrated by his daughter

Constance. Regarding this he says in his preface :—"The object of this little book is to give in as simple a form as possible a short account of some of the British Wild Bees, Wasps, Ants, etc., scientifically known as the *Hymenoptera Aculeata*. Of these the non-scientific public rarely recognizes more than the Hive Bee, the Humble Bee, the Wasp, and the Hornet, whereas there are about 400 different kinds to be found in this country, and they can be recognized by any one who is disposed to make a special study of the group."

To the Linnean Society Edward Saunders contributed a paper read Nov. 7, 1867, published April 23, 1868, communicated by his father, then a V.P.L.S. The subject was "Descriptions of fifty new Species of the genus *Stigmodera*," with two plates drawn by the author. Another paper containing "Descriptions of ten new Species of the genus *Paracupta*, H. Devrolle, and of ten new Species of the genus *Conognatha*, Escholtze," with one plate, was read Dec. 3, 1868, and published Aug. 10, 1869, the author having in the meantime, on June 3rd, been elected a Fellow. His third paper, "Descriptions of Buprestidae collected in Japan by George Lewis, Esq." (F.L.S. since Jan. 18, 1883), was read Feb. 20, 1873, and published July 18 of the same year. Thirty-four new species, with a new genus, were included in this memoir. After a long interval he again contributed to the Linnean Journal, his paper "On the Tongues of the British Hymenoptera Anthophila" being read April 17, 1890, and published Oct. 18 of that year. These honeyed Tongues, successfully mounted by the ingenious Mr. Frederick Enock, F.L.S., were delineated on eight attractive plates by George Saunders, who in 1882 had similarly assisted his younger brother by drawing plates for his "Synopsis of British Hymenoptera" in the 'Transactions' of the Entomological Society.

While treating the biography of the two brothers chiefly from its scientific aspect, it may not be improper for the recorder here to note that in 1873 the prospects of easy affluence with which their careers had opened were clouded over. This change from the smiles of fortune to her frowns they met with brave equanimity. It left the younger immersed for the rest of his life in the business anxieties of marine insurance, while the elder suffered for some time from the unsettlement due to abruptly altered plans and avocations. These were henceforward considerably diversified, but their general character will be sufficiently understood by the following extracts from obituary notices published last April.

Thus 'The Garden' says: "Mr. George Saunders was a warm lover of flowers, and also took an interest in insects, so much so that we are sorry no book came from his pen on this important subject. His initials G.S.S. were familiar, until his recent illness, to readers of 'The Garden,' and no one was more qualified to give advice on the friends and foes of the garden. One of his most important contributions on this subject was to the 'Century Book of Gardening,' and he contributed an excellent series of

articles in 'Gardening Illustrated' some years ago."... "Our friend was librarian for many years at St. Thomas's Hospital, and many medical men will feel a real pang of sorrow when they know that Mr. Saunders is dead. He was beloved by the students and the staff generally." The 'Gardeners' Chronicle' says: "He was soon initiated in horticulture, but his bent lay more towards the study of insects than of other living things, though he took a keen interest in many branches of natural science. He became well known as an expert in economic entomology, and his opinion was on this subject widely sought and greatly valued, by none more than by the Scientific Committee of the Royal Horticultural Society, whose meetings he regularly attended for many years. His knowledge of entomology was always at the disposal of enquirers, and his ready courtesy and clear expositions of life-histories and so on made his communications of real value. He was the author of several clearly written articles (not all of them signed) in the horticultural Press. He was a skilled draughtsman, and made a considerable number of coloured drawings of teratological subjects of great interest and value to the botanist. When in 1906, the Rev. W. Wilks was obliged, through pressure of work, to give up the editorship of the R.H.S. Journal, Mr. Saunders was appointed to succeed him, and he edited the Journal until 1908, when ill-health obliged him to resign."

George Saunders had been a Fellow of the Entomological Society from 1861 and had served on its Council. It was not till 1899 that he joined the Linnean Society, on the Council of which he also served from 1902 to 1905, his appointment coinciding with his brother's election as a Fellow of the Royal Society.

Alike in their devotion to natural history, the two brothers were further alike in a certain seriousness of temper which evidently invited appeals for their aid in church-work wherever they happened to reside. This earnestness was happily combined with a ready sense of things humorous and with what may best be described as a singular capacity for friendship.

For many years of happy married life they were alike in enjoyment and gratitude. But George Saunders, who was united to Miss Mary Horsley on July 9, 1868, had the sorrow of losing her, after long and painful illness, in 1909, whereas Miss Mary Agnes Brown, to whom Edward Saunders was married Sept. 3, 1872, survives him, together with nine of their twelve children, most of them already engaged in a variety of promising pursuits. The brothers died as they had lived, each fading from the scene with a kind of modest tranquillity, Edward on the 6th of February last, and George on the following 6th of April. Both had been for some time conspicuously out of health, yet in each case there seemed room for hope, though from the opposite considerations that the one had been so seldom ill and the other so often. The elder, after a delicate childhood, had proved immune to sickness, till sympathy with his wife's affliction apparently broke down his powers of resistance. The younger, on the other hand, had so



frequently recovered from dangerous attacks that there seemed no special reason why the last should be fatal. Yet they fell, not indeed on one day, nor on the field of battle, but after so close a union from first to last, that, borrowing from a song of triumphant sorrow, a friend may say of them, "They were lovely and pleasant in their lives, and in their deaths they were not divided."

[T. R. R. STEBBING.]

The death of Dr. RICHARD BOWDLER SHARPE, which took place at his residence at Chiswick on Christmas Day 1909, deprived Ornithologists, the world over, of a guide, philosopher, and friend indeed; for his knowledge of Systematic Ornithology, and of the Geographical Distribution of Birds and all that pertained thereto, was profound. So long as he lived this knowledge was at the disposal of his fellow-workers without reserve; for he was one of the most generous of men, and no man turned away from him empty handed. But when he died a rich hoard of facts died with him, for in spite of his extraordinary output of memoirs and monographs, the best of what he knew he could never be induced to systematize and publish.

Dr. Sharpe was born in London, November 22, 1847, and was the eldest son of Thomas Bowdler Sharpe, well known as the publisher of 'Sharpe's London Magazine.' His grandfather was the Rev. Lancelot Sharpe, Rector of All Hallows Staining, in the City, and for many years Headmaster of St. Saviour's Grammar School in Southwark. Happily he was not brought up in London, but at the age of six was placed under the care of his aunt, Mrs. Magdalen Wallace, widow of the Rev. J. Wallace, Headmaster of the Grammar School at Sevenoaks. She kept a preparatory school at Brighton, and here the boy passed three uneventful years; he was then transferred to the Grammar School at Peterborough, where his cousin, the Rev. James Wallace was Master. Here he gained a King's Scholarship, which not only guaranteed his education but carried with it a small sum of money which was increased by his services as a choir-boy in the Cathedral. A little later his cousin accepted the Headmastership of the Grammar School at Loughborough, and the boy accompanied him. In these sojournings young Sharpe found scope for his innate love of Natural History, which was to bear such fruit in after years. But a time of trial was before him. An unsympathetic father, irritated at this marked fondness for what he regarded as an unprofitable subject boding no good for the future, suddenly bundled him off to London—a boy of sixteen—with a sovereign in his pocket, and a letter of introduction to the publishing firm of W. H. Smith & Sons! But opposition of this kind rarely attains its end. It certainly did not in the present case: on the contrary, it seems to have added fuel to the flames; and the boy succeeded, in spite of this disaster, in following his bent, for here, though every imaginable obstacle confronted him, he began to write a Monograph of the Kingfishers which marked an epoch



in Ornithological Literature. Two years later he entered the employment of Mr. B. Quaritch, the well-known bookseller, and this gave him access to some of the finest Ornithological works of the time. Every penny he could scrape together he spent in buying specimens of Kingfishers for his great book, and every moment he could spare was spent in its preparation, so that his working hours were long indeed.

In 1867, when nineteen years old, he was appointed Librarian to the Zoological Society of London—the first to hold this position, and this appointment gave him better opportunities, and greater scope for his talents. Five years later the reward for his years of unnecessary hard labour came to him, for in 1872 he was appointed Senior Assistant in the Department of Zoology of the British Museum, and was placed in charge of the Collection of Birds.

One of the first tasks entrusted to him was the preparation of the first volume of that monumental work the British Museum ‘Catalogue of Birds,’ the most exhaustive work of its kind in existence. In the seven-and-twenty volumes of that work every known species of bird, up to the date of publicity of each volume, is described. This entailed an account of all phases of plumage and a list of all the literature, relating to each species—a truly appalling task; yet of these twenty-seven volumes Sharpe contrived to write eleven, and portions of two others. At least this much stands to his credit: the remaining volumes were written in part by Mr. W. R. Ogilvie-Grant, and in part by outside aid, and these later authors drew largely on Sharpe for guidance and help.

But besides this, he also compiled during his later years a ‘Hand-list of Birds,’ the last volume of which was only completed just before his death. Most men would have quailed before such an undertaking requiring so much labour and yielding so little in return, for it can appeal only to the specialist.

Another official publication was his ‘History of the Bird Collection in the British Museum.’ This is full of interesting matter concerning the collections, and the men who made them.

Besides his Monograph of the Kingfishers he also wrote no less comprehensive treatises on the Swallows and the Birds of Paradise, and a Handbook to the Birds of Great Britain. Innumerable appendices to the Volumes of Travel written by eminent explorers, describing the birds collected on their journeys, and a no less voluminous list of separate reports and papers on new species of birds, in themselves form no mean monument. Besides, he also edited, or revised and largely re-wrote, a considerable number of works of eminent Ornithologists removed by death before their labours were complete. The huge tomes of John Gould, and two volumes of Henry Seebohm are among the most notable of this category. To these we must add a very long list of papers contributed to the Journal and Transactions of this Society, the Zoological Society, the ‘Annals and Magazine of Natural History,’ ‘The Ibis,’ and the Bulletin of the British Ornithologists’ Club.

In his later years he edited a two volume edition of White's *Selborne*, and during his researches in the Gilbert White country he became interested in the history of Basing Castle and the story of the siege by Cromwell. This theme he pursued with great zeal, and finally decided to write a book on the subject. Unhappily he died while preparing the first volume, but had he lived to complete his task, it is certain that he would have produced a record of sterling merit, for I had the good fortune to know him intimately, and know how exhaustively he had treated his subject. But this was by way of recreation, though his official work left him but little real leisure.

Sharpe always regarded the Collections under his charge with a peculiarly tender affection, and the main purpose of his life was to enrich them. He spared no effort to persuade the jealous Guardians of the Museum purse to buy collections, and often when his pleadings proved unavailing he would purchase collections with his own money—which he could ill afford to do, but it was anguish unspeakable to him to let a specimen escape that he courted for his beloved collection. He had great powers of persuasion, and these he exerted to the full when he desired to move some generously inclined traveller to hand over his specimens to the national store-house. The great Hume Collection of Indian birds, and the wonderful Tweeddale and Salvin-Godman collections are among the more remarkable illustrations of his triumphs, for these were given to the Museum largely on Sharpe's account. Mr. Hume, in presenting his magnificent collection of Indian birds and eggs numbering nearly 80,000 specimens, remarked in presenting the Collection to the Nation: "I trust it may not be forgotten that its acquisition by the Museum has been solely due to the fact that Mr. Sharpe was at the head of the Ornithological branch of that Institution." But this is only one, of many munificent gifts to the Ornithological Department made on Sharpe's recommendation.

He was President of Section A at the Ornithological Congress held at Budapest in 1891, and again in Paris in 1900, when he was elected President of the Congress which met in London five years later. He was an Honorary LL.D. of Aberdeen, a Fellow of the Linnean and Zoological Societies, a Member of the British Ornithologists' Union, and was an Honorary or Foreign Member of all the principal Ornithological Societies in the world. In 1891, H.I.M. the Emperor of Austria bestowed on him the Gold Medal for Science.

His rule as Assistant Keeper of the Zoological Department was mildness itself, for he was of a peculiarly genial temperament. In spite of domestic worries he was always in exuberant spirits, and was a wonderful story-teller. His friends loved to inveigle him into telling yarns of the many eccentric people he had met, or into reciting Bab-ballads, in which feat he was peculiarly accomplished; few, surely, ever succeeded in bringing out the exquisite humour of these lines so well as Sharpe! But his kindly

nature was sorely abused by people of the begging-letter type : he could never resist an appeal to his purse, though that was far from a well-filled one, and he was no less generous in bestowing the fruits of scientific labours on those who asked him.

Take him for all in all, we shall not see his like again.

[W. P. PYCRAFT.]

Sir CHARLES WILLIAM STRICKLAND, eighth baronet, who died on the last day of 1909, Dec. 31st, was a Fellow of the Linnean Society from the 15th February, 1877. He resided at Hildenley Hall near Malton, on one of his country estates, for many years. The Hall, although not a very large building, is a comfortable residence, built in a well sheltered site at the base of a steep wooded bank of limestone formation known as Hildenley Wood, which is a relic of the ancient forest-land of Yorkshire and has never been under cultivation, and is the home of some of the rarest of our British native orchids and other rare kinds of the wild flora of Britain. He was the original of "Martin the Madman" in 'Tom Brown's Schooldays,' and was proud of the fact.

Sir Charles was a lover of Nature, and lived for many years a quiet life. He built attached to the Hall a fine conservatory for flowering plants, and also erected other glass structures for the cultivation of exotic orchids, in which he was very successful, and gave much attention to them, and no doubt enjoyed his quiet life in the study of their growth and admiration of the great beauty and variety of their flowers.

He was a good botanist and had an extensive knowledge of the flora of the British Islands ; he was also much interested in the cultivation of hardy fruits, more especially of the apple, as he considered it to be the most valuable fruit for general cultivation in England. Some twenty years ago he represented a committee of the Royal Horticultural Society, in the examination of the apples growing in this district of Yorkshire, more particularly to get a knowledge of the best kinds suitable to the locality. For two or three seasons collections of these fruits were exhibited at Malton and Whitby, local kinds were traced to their origin, and all exhibits were named as far as possible and lists given of those most suitable to the district.

He was a large landowner in Yorkshire and generally considered to be a generous landlord ; he let his farms at reasonable rents, and very rarely changed his tenants.

He was a tall, robust man of good appearance, was a careful liver, and enjoyed the life of a country gentleman. He was fond of hunting, and was a regular attendant at Lord Middleton's hunt during the season up to within a few years of his death. In his early life, whilst he was at Trinity College, Cambridge, he rowed at Henley Regatta in 1839, the first year the grand challenge cup for Eights was ever rowed for, and was number 7 in the winning crew, his college eight.

Sir Charles was twice married: his first wife was Georgina Milner, daughter of Sir William Milner, by whom he had one son, Walter William Strickland, born in 1851, who now succeeds to the title and estates, and who has been travelling for some years in the Colonies of Australia, New Zealand, India, Ceylon, &c., and is now residing in Italy. The second marriage was in 1866, to Ann Elizabeth, daughter of the Rev. Christopher Neville of Thorney, Notts, who predeceased him. There are two surviving sons and one daughter of the second family, Mr. Frederick Strickland, of the Brows, Malton, and Lieut. Henry Strickland, of the Royal Navy. His daughter is married to the Hon. Tatton Lane Fox Willoughby.

Sir Charles had a good constitution and enjoyed the happiness of good health through his long life, and passed away quietly, after a very short illness, in his home at Hildenley, in the 91st year of his age. [M. B. SLATER.]

The death of EDWARD PERCEVAL WRIGHT, which took place on March 4th, caused sincere regret among the large group of friends and acquaintances whose affection and regard Wright's genial and striking personality had won. Wright was born in Dublin in 1834. He was educated at home and began life as a clerk at the age of 16. His taste for Natural History brought him early into contact with Geo. J. Allman, the University Professor of Botany in Trinity College, Dublin, and resigning his clerkship, he entered Trinity College in 1853 and threw himself with zeal into the study of Natural History. His enthusiasm to forward the science led him in the following year to found the 'Natural History Review.' He continued editor of that Journal till 1866. His earliest essays in Science were made in this Journal and exhibit his keen interest from the first in field-work and observation. He visited the caves of Michelstown in 1857, with Haldiday, and was the first to point out the interest of the living fauna of the Irish caves. In the same year, he took his B.A. degree and was appointed Director of the Museum of Trinity College. In 1858, he was appointed Lecturer in Zoology in Trinity College and Lecturer in Botany in Dr. Steevens' Hospital Medical School, and he was chosen Secretary for the Royal Geological Society of Ireland and Secretary for Section D of the British Association. He continued Secretary of this Section for several years, and was always very much interested in the work of the Association. It is a proof of Wright's energy and capacity for work, that while he was discharging all these duties he was at the same time able to keep pace with his undergraduate medical studies, and did not give up his work in Natural Science. He was elected Fellow of the Linnean Society in 1859. In 1862 he took his M.D. degree, and after studying abroad in Berlin, Vienna, and Paris, he began to practise as an oculist. But his chief interests being in Natural Science he did not remain in practice long, and in 1866, in order to devote himself entirely to his duties as *locum tenens* for Harvey, then the University Professor



of Botany. Wright definitely gave up ophthalmological work. In 1865 his attention was called to the fossils of the Jarrow Colliery, and with T. H. Huxley he published in the following year an important memoir on the Fossil Amphibia of the Kilkenny Coal Measures.

In 1867, Wright went to the Seychelles Islands to study the flora and fauna of that group. Unfortunately, all his collecting apparatus and preserving materials were lost on the way out by shipwreck. But notwithstanding this misfortune he succeeded in bringing back an important collection of animals and plants, and in the following years was able to publish a series of papers describing the new and interesting forms collected. These papers, together with others on collections made off the coast of Portugal and in Sicily in 1868, appeared in the 'Annals and Magazine of Natural History,' in the Transactions of the Royal Irish Academy, and in the Reports of the British Association.

In 1869, Wright was appointed University Professor of Botany and Keeper of the Herbarium in Trinity College. During the first years of his professorship he was still engaged in work on his Seychelles and South European collections. In 1877, Wright commenced to publish a series of memoirs on the structure and development of Algæ, which won the appreciative recognition of Bornet in 1879. The work on Algæ was put aside to draw up the Report, with Th. Studer, on the Alcyonaria of the 'Challenger' Expedition. This was not completed till 1888. During this period also Wright spent a great deal of energy on arranging the Herbarium of Trinity College; and if it had not been for his devotion and painstaking toil at a critical time, the usefulness of the collection would have been seriously impaired. The history of these events he records in the first number of the 'Notes from the Botanical School of Trinity College'—a journal which owes its existence to Wright's energy and generosity. He further showed his affection to the department of which he was head by presenting to it his valuable collection of botanical books and journals. In 1894 Wright visited the Pyrenees and brought back several additions to the Herbarium, and the spring of 1895 he spent collecting in Algiers. In 1904 he resigned his professorship after a tenure of 35 years. He remained Keeper of the Herbarium till his death.

In addition to his researches on Distribution and on Systematic Biology, Wright took an active part in many Scientific Societies, and was officially connected with several scientific publications. Among these activities may be mentioned his connection with the 'Natural History Review' as founder and editor, as Secretary, with the Dublin University Zoological and Botanical Association, with the Royal Geological Society of Ireland, and with Section D of the British Association. He was President of the Natural History Society of Dublin in 1872, and in 1874 he became Secretary of the Royal Irish Academy and editor of its publications. He was also editor for some time of the publications of the Royal

Dublin Society. In these various capacities he showed remarkable energy in forwarding the welfare of the institutions with which he was connected, and his generosity, when need arose, could always safely be counted upon. He displayed the same activity and generosity towards antiquarian research, and the Royal Society of Antiquaries marked its appreciation of his services in forwarding its aims by electing him President in 1900.

Wright's sympathetic nature won the affection of those who came in contact with him, and he was keenly desirous of forwarding younger men's work in science, and generously helped them by all the means in his power. It was a pleasure to him to put his varied and often recondite knowledge of the literature of Natural Science at their disposal. He showed the liveliest appreciation of the results obtained by the more modern generation of biologists. As a teacher he was more than ordinarily successful in stimulating the enthusiasm of his students and in implanting in them the desire to carry out investigation. At the same time he had a keen interest in his contemporaries in scientific work, and his desire to help the work of others and his human sympathies brought him into personal contact with a large number of his colleagues, not only in the British Isles, but also on the Continent and in America. It was always a pleasure to him to speak of these friends, and anecdotes of them formed a feature of his conversation. [H. H. DIXON.]

June 2nd, 1910.

Dr. D. H. SCOTT, M.A., F.R.S., President, in the Chair.

The Minutes of the Anniversary Meeting of the 24th May, 1910, were read and confirmed.

Miss Nellie Bancroft, Mr. Sidney Guest, and Mr. Hayward Radcliffe Darlington, M.A., LL.M.(Cantab.), were admitted Fellows.

Mr. Anthony Belt, and Prof. Edward Alfred Minchin, M.A.(Oxon.), were proposed as Fellows.

Mr. Cecil Hanbury, Mr. Henry Smith Holden, B.Sc., Mr. Charles William Mally, M.Sc.(Iowa), Mr. Sydney Gross Paine, and Mr. Percy Alfred Talbot, B.A.(Oxon.), were elected Fellows.

The President stated that he had appointed Sir Frank Crisp, Mr. H. W. Monckton, Prof. F. W. Oliver, and Prof. E. B. Poulton, to be Vice-Presidents for the ensuing Session.

Mr. H. W. MONCKTON, Treasurer and V.-P., then referred to previous exhibitions of Witches' brooms or Witch-knots in Conifers; instancing those by Dr. Masters on 18th March, 1886,

Mr. James Saunders, A.L.S., on 21st April, 1907, and the Rev. T. R. R. Stebbing on 21st April of the present year. He showed by lantern-slides similar growths on *Pinus sylvestris* growing near Wellington College, in Berkshire.

The President pointed out that the term was probably a recent translation of the German "Hexenbesen"; and the discussion was continued by Miss A. L. Smith, Mr. J. C. Sheenstone, the General Secretary, Dr. A. P. Young, and Dr. A. B. Rendle.

Dr. STAFF, on behalf of Mr. J. F. WABY, F.L.S., of the Botanic Garden, Georgetown, British Guiana, exhibited lantern-slides from photographs of male and female specimens of *Lodoicea Sechellarum*, Labill., which were flowering and fruiting in that Garden.

He stated that of 36 nuts specially imported in 1893, only three plants survived, the two in question, and a third which had not yet flowered. It is of interest as being the first occasion of this palm flowering in the New World, and for its precocious development.

The President, Dr. Rendle, and Mr. W. Fawcett contributed further remarks.

Sir FRANK CRISP showed fresh specimens in flower of *Linnaea borealis* from his garden at Friar Park, Henley.

The General Secretary placed on the table for inspection, a living specimen of the rare and local orchid, *Ophrys aranifera*, received that morning from Mrs. Mann, of Temple Ewell Vicarage, near Dover; it had been procured from the neighbourhood of Folkestone.

The following paper was read:—

"A Contribution to our Knowledge of the Flora of Gazaland, an Account of Collections made by Mr. Swynnerton."  
By Dr. A. B. RENDLE and others.

June 16th, 1910.

Dr. D. H. SCOTT, M.A., F.R.S., President, in the Chair.

The Minutes of the General Meeting of the 2nd June, 1910, were read and confirmed.

Mr. Percy Alfred Talbot, B.A.(Oxon.), and Mr. Sydney Gross Paine were admitted Fellows.

Dr. Edward Hindle and Mr. Cuthbert St. John Nevill were proposed as Fellows.

Dr. Wilfred Eade Agar, M.A.(Cantab.), was elected a Fellow.

The President read a letter to Sir JOSEPH HOOKER, O.M., G.C.S.I., F.R.S., congratulating him on the approach of his 93rd birthday, which was signed by the Fellows present.

Prof. A. DENDY, Sec.L.S., on behalf of Mr. N. C. MACNAMARA, F.R.C.S., of Chorley Wood, Herts, showed a spike cut from a Foxglove grown from seed of a sport which appeared in 1907, in which the corolla was suppressed but the five divisions were represented as stamens, making nine in all, and this peculiarity comes true from seed.

Lantern-slides showing the original mutation and other descendants thereof were also shown; and a discussion followed, in which Dr. Stapf (who pointed out the frequency of monstrosities in *Digitalis purpurea*), Mr. J. C. Shenstone, Mr. Arthur W. Sutton, and the President engaged.

Mr. J. HOPKINSON showed under the microscope a slide by Dr. Penard, of Geneva, illustrating a peculiar method of reproduction in freshwater Rhizopoda, two specimens uniting to form a third of larger size than either, ultimately giving rise to spores.

The President remarked on the similarity between these Protozoa and such Conjugata as *Spirogyra* and *Mesocarpus*, showing that these lowly organisms should be studied by both botanists and zoologists.

Mr. P. A. TALBOT exhibited a large series of coloured drawings by Mrs. TALBOT of plants from Southern Nigeria, and displayed a map and photographs of the scenery. He described the country as very hilly and densely wooded.

"These photographs are of the Kwa River and give some idea of the beauty and density of the vegetation, but none of the glory of colouring or variety of the multitudes of flowers. Right down to the water's edge grow giant arums, green on the outer sheath, but cream splashed with purple within. Behind these spring trees of every shape and tint, from mimosas, with their delicate mauve or cream balls and feathery foliage, to the huge trumpet-shaped flowers of *Gardenia physophylla*, and the heavily scented purple-splashed blooms of *G. Kalbreyeri*, or the great *Berlinia*, the white flowers of which shine with a pearl-like lustre from amid its dim dark leaves.

About this river lies the boundary between the sedimentary deposits below and the crystalline rocks above. The line of demarcation runs along this parallel to the Akwa Yafe on the German Border, and the Calabar River on the other side. By far the greater part of the District therefore is composed of metamorphic rocks in which gneiss predominates.



The photographs show the density of the bush through which the roads lead. Perhaps the most striking feature of all in these ancient forests is the hurry shown by all trees to reach the light, above the thick undergrowth.

Perhaps the tallest of all the bush giants are the silk-cotton trees. It is difficult to get a good photograph of these owing to the density of the surrounding bush, which would have to be cleared for a great way before a picture could be taken. The photograph is of a comparatively poor specimen, which stood on the edge of a clearing. It is only about 150 feet high. The man standing at the base was the tallest carrier available, a man well over 6 feet. These trees are often 200 to 250 feet high, and have a girth of over 80 feet.

Another photograph shows the source of the Calabar River. It was on the slope of a hill near by that a *Napoleona* was discovered, which is not only a new species, but which shows an inflorescence hitherto unknown in this interesting genus. Altogether, four new *Napoleonas* have been brought home—thus adding half as many again to those already known. The second, with the consent of the courteous authorities at the Natural History Museum, has been named after my friend and former leader, Boyd Alexander, who was murdered on April 2nd in Central Africa.

Altogether over fifty specimens of cauliflorous trees were discovered in the District. Detailed drawings of all these were made, but unfortunately many of the actual specimens were ruined by climatic conditions or lost in transit. This number only represents a small proportion of those to be found. I hope to bring back at least double the number on my next tour.

Of the *Balanophoraceæ*, five species have been brought home. The *Gardenias* of the district are specially striking in the size and beauty of their flowers. The fruits of most of them afford excellent black dyes, some of which are at present being tested at the Imperial Institute, and also a new fibre, made from an epiphytic *Arum*, which I forwarded with them.

Two kinds of *Geasters* were found in the District. These are the first of this genus discovered in Africa. The specimens are in England, but have been temporarily mislaid. Altogether over a thousand drawings were made in the course of the year."

Dr. Rendle, Dr. Stapf, Mr. E. G. Baker, and Mr. J. Hopkinson discussed the exhibition, and Mr. P. A. Talbot replied.

Dr. STAFF showed a selection of Arctic specimens collected by Capt. Bartlett during the last Peary Expedition, on Ellesmere Island, between 82° and 83° N. latitude, describing them as some of the most northerly botanical specimens extant.

Mr. A. W. HILL showed a specimen in spirit of a barren stem of *Equisetum Telmateia*, Ehrh., in which about half of the nodes

disappeared in a spiral arrangement; it had been sent by Dr. H. Drinkwater, F.L.S., of Wrexham.

Dr. Rendle made a few remarks on this phenomenon.

The following paper was read:—

“Male Sterility in Potatoes, a dominant Mendelian character; with Remarks on the Shape of the Pollen in Wild and Domestic Varieties.” By Dr. R. N. SALAMAN. (Communicated by Mr. A. W. SUTTON, F.L.S.)

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# ADDITIONS AND DONATIONS

TO THE

## LIBRARY.

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- Arrow (G. J.).** See **Blanford (W. T.).** The Fauna of British India, including Ceylon and Burma. Coleoptera—Lamellicornia (Cetoniinæ and Dynastinæ). 8vo. 1910.
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1861.

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Collection of birds' eggs, bequeathed by John Drew Salmon, F.L.S.

1862.

The Linnean Club: presentation bust of Prof. T. Bell, by P. Slater.

1863.

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1864.

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1865.

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George Bentham, Esq.: cost of 10 plates for his "Tropical Leguminosæ," Trans. vol. xxv.

1866.

Dr. Friedrich Welwitsch: Illustrations of his 'Sertum Angolense,' £130.

1867.

George Bentham, Esq.: General Index to Transactions, vols. i.-xxv.  
Royal Society: Grant in aid of G. S. Brady on British Ostracoda, £80.

1869.

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1874.

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1875.

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1876.

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1878.

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1879.

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The Secretary of State for India in Council : Grant for Delimitation  
Expedition report, £200.

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John Ball, £60.

Subscription portrait of Professor George James Allman, by  
Marian Busk.

1898.

Sir John Lubbock, Bt. : Contribution towards his paper on  
Stipules, £43.14s. 9d.

Royal Society : Contribution towards F. J. Cole's paper, £50.

" " " " " " Murray & Blackman's paper,  
£80.

" " " " " " Elliot Smith's paper, £50.

" " " " " " Forsyth Major's paper, £50.

## 1899.

A. C. Harmsworth, Esq. [Lord Northcliffe]: Contribution towards cost of plates, £43.

Royal Society: Contribution towards Mr. R. T. Günther's paper on Lake Urmi, £50.

## 1901.

Hon. Charles Ellis, Hon. Walter Rothschild, and the Bentham Trustees: The Correspondence of William Swainson.

Royal Society: Contribution towards Mr. F. Chapman's paper on Funafuti Foraminifera, £50.

Prof. E. Ray Lankester: Contribution towards illustration, £30 5s. Portrait of Dr. St. G. J. Mivart, presented by Mrs. Mivart.

## 1903.

Royal Society: Contribution toward Dr. Elliot Smith's paper, £50. Legacy from the late Dr. R. C. A. Prior, £100 free of duty.

Mrs. Sladen: Posthumous Portrait of the late Walter Percy Sladen, by H. T. Wells, R.A.

B. Arthur Bensley, Esq.: Contribution to his paper, £44.

## 1904.

Royal Society: Grant in aid of third volume of the Chinese Flora, £120.

Supplementary Royal Charter: cost borne by Frank Crisp, Esq. (afterwards Sir Frank Crisp).

## 1905.

Royal Society: First grant in aid of Dr. G. H. Fowler's 'Biscayan Plankton,' £50.

Executors of the late G. B. Buckton, Esq.: Contribution for colouring plates of his paper, £26.

## 1906.

Royal Society: Second grant towards 'Biscayan Plankton,' £50.

Subscription portrait of Prof. S. H. Vines, by Hon. John Collier.

Royal Swedish Academy of Science: Copies of portraits of C. von Linné, after Per Krafft the elder, and A. Roslin, both by Jean Haagen.

## 1907.

Royal University of Uppsala: Copy by Jean Haagen of portrait of C. v. Linné, by J. H. Scheffel (1739).

Royal Society: Third and final grant towards 'Biscayan Plankton,' £50.

The Trustees of the Percy Sladen Memorial Fund: First grant towards publication of Mr. Stanley Gardiner's Researches in the Indian Ocean in H.M.S. 'Sealark,' £200.

## 1908.

Prof. Gustaf Retzius: Plaster cast of bust of Carl von Linné, modelled by Walther Runeberg from the portrait by Scheffel (1739) at Linne's Hammarby: the bronze original is for the façade of the new building for the Royal Academy of Science, Stockholm.

Miss Sarah Marianne Silver, F.L.S.: Cabinet formerly belonging to Mr. S. W. Silver, F.L.S.

## 1909.

The Trustees of the Percy Sladen Memorial Fund: Second grant towards publication of Mr. Stanley Gardiner's *Researches in the Indian Ocean in H.M.S. 'Sealark,'* £200.

Prof. James William Helenus Trail, F.R.S., F.L.S.: Gift of £100 in Trust, to encourage Research on the Nature of Proto-plasm.

## 1910.

Royal Society: Grant towards Dr. G. H. Fowler's paper on *Biscayan Ostracoda*, £50.

Sir Joseph Hooker: Gold watch-chain worn by Robert Brown, and seal with portrait of Carl von Linné by Tassie.

[With the gifts received in 1888, the Society now possesses the gold watch and chain formerly belonging to Robert Brown, with the watch-key, amethyst signet engraved R. B., cornelian signet engraved J. D. = Jonas Dryander, and cornelian seal, with Linnean bust engraved after C. F. Inlander by William (?) Tassie.]

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- Worster-Drought, C., elected, 7; proposed, 1.
- Wrexham, drawings of wild flowers from, exhibited (Drinkwater), 7, 53.
- Wright, E. P., deceased, 61; obituary, 102-104.
- Yerbury, Lt.-Col. J. W., withdrawn, 64.
- Yew trees in Norton Churchyard, mentioned, 2.
- Zannichellia*, photographs of, exhibited (Reid), 8.
- *gibberosa*, exhibited (Druce), 7.
- Zizyphus Jujuba* exhibited (Holmes), 57.
- Zoological Secretary elected, 65.

The following Councillors retired at the Anniversary Meeting,  
24th May, 1910 :—

L. A. BOODLE, Esq., R. I. POCOCK, Esq., Lieut.-Col. PRAIN,  
Miss ETHEL SARGANT, and Prof. A. C. SEWARD.









Publications of the Society issued during the period, 31st July, 1909, to 31st July, 1910:—

Journal (Botany), No. 270, 4th Aug., 1909.  
„ 271, 28th Oct., 1909.  
„ (Zoology), No. 200, 15th Dec., 1909.  
„ 201, 22nd June, 1910.  
„ 206, 30th Nov., 1909.

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Transactions (2nd Ser.) Botany, Vol. VII. Part XIII., Sept. 1909.  
„ XIV., Nov. 1909.  
(2nd Ser.) Zoology, Vol. X. Part IX., Nov. 1909.  
„ XIII. „ I., Oct. 1909.  
„ II., Feb. 1910.  
„ III., June 1910.

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Proceedings, 121st Session, from November 1908 to June 1909 :  
October 1909.

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List of [Fellows, Associates, and Foreign Members], 1909-1910.